PROCEEDINGS

OF

THE AMERICAN ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE

FORTY-SIXTH MEETING

HELD AT

DETROIT, MICH.

AUGUST, 1897.

SALEM:
PUBLISHED BY THE PERMANENT SECRETARY.
JUNE, 1898.

EDITED BY

FREDERIC W. PUTNAM,
Permanent Secretary.

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OFFICERS

OF THE

DETROIT MEETING.

PRESIDENT.

WOLCOTT GIBBS, Newport.

W J McGEE, Washington, *Acting President.*

In the absence of the President, the Senior Vice President became the acting President and presided throughout the meeting.

VICE PRESIDENTS.

A. Mathematics and Astronomy—W. W. BEMAN, Ann Arbor, M.
B. Physics—CARL BARUS, Providence, R. I.
C. Chemistry—W. P. MASON, Troy, N. Y.
D. Mechanical Science and Engineering—JOHN GALBRAITH, Toronto, Canada.
E. Geology and Geography—I. C. WHITE, Morgantown, W. Va.; E. W. CLAYPOLE, Akron, Ohio, was elected Vice President and presided in the absence of Professor White.
F. Zoölogy—G. BROWN GOODE, Washington, D. C. (*Deceased.*) L. O. HOWARD, Washington, was elected to fill vacancy.
G. Botany—GEORGE F. ATKINSON, Ithaca, N. Y.
H. Anthropology—W J McGEE, Washington, D. C.
I. Social and Economic Science—RICHARD T. COLBURN, Elizabeth, N. J.

PERMANENT SECRETARY.

F. W. PUTNAM, Cambridge, Mass. (Office Salem, Mass.)

GENERAL SECRETARY.

ASAPH HALL, JR., Ann Arbor, Mich.

SECRETARY OF THE COUNCIL.

D. S. KELLICOTT, Columbus, Ohio.

SECRETARIES OF THE SECTIONS.

A. Mathematics and Astronomy—JAMES McMAHON, Ithaca, N. Y.
B. Physics—FREDERICK BEDELL, Ithaca, N. Y.
C. Chemistry—P. C. FREER, Ann Arbor, Mich.
D. Mechanical Science and Engineering—JOHN J. FLATHER, Lafayette, Ind.
E. Geology and Geography—C. H. SMYTH, JR., Clinton, N. Y.
F. Zoölogy—C. C. NUTTING, Iowa City, Iowa.
G. Botany—F. C. NEWCOMBE, Ann Arbor, Mich.
H. Anthropology—HARLAN I. SMITH, New York, N. Y. (*Resigned.*) ANITA NEWCOMB McGEE, Washington, D. C., was elected to fill vacancy.
I. Social and Economic Science—ARCHIBALD BLUE, Toronto, Canada.

TREASURER.

R. S. WOODWARD of New York, N. Y.

MEMBERS OF THE COUNCIL

FOR THE

DETROIT MEETING.

Past Presidents. — JAMES HALL of Albany; SIMON NEWCOMB of Washington; O. C. MARSH of New Haven; GEORGE F. BARKER of Philadelphia; GEORGE J. BRUSH of New Haven; J. W. DAWSON of Montreal; C. A. YOUNG of Princeton; J. P. LESLEY of Philadelphia; EDWARD S. MORSE of Salem; SAMUEL P. LANGLEY of Washington; J. W. POWELL of Washington; T. C. MENDENHALL of Worcester; GEORGE L. GOODALE of Cambridge; ALBERT B. PRESCOTT of Ann Arbor; JOSEPH LECONTE of Berkeley; WILLIAM HARKNESS of Washington; DANIEL G. BRINTON of Media; EDWARD W. MORLEY of Cleveland.

Vice Presidents of the Buffalo Meeting. — ALEX. MACFARLANE of S. Bethlehem; CARL LEO MEES of Terre Haute; W. A. NOYES of Terre Haute; FRANK O. MARVIN of Lawrence; BEN. K. EMERSON of Amherst; THEODORE GILL of Washington; N. L. BRITTON of New York; ALICE C. FLETCHER of Washington; WILLIAM R. LAZENBY of Columbus.

Officers of the Detroit Meeting. — WOLCOTT GIBBS of Newport (absent); W. W. BEMAN of Ann Arbor; CARL BARUS of Providence; W. P. MASON of Troy; JOHN GALBRAITH of Toronto; E. W. CLAYPOLE of Akron; L. O. HOWARD of Washington; GEORGE F. ATKINSON of Ithaca; W J MCGEE of Washington; RICHARD T. COLBURN of Elizabeth; F. W. PUTNAM of Cambridge; ASAPH HALL, JR. of Ann Arbor; D. S. KELLICOTT of Columbus; JAMES MCMAHON of Ithaca; FREDERICK BEDELL of Ithaca; P. C. FREER of Ann Arbor; JOHN J. FLATHER of Lafayette; C. H. SMYTH, JR. of Clinton; C. C. NUTTING of Iowa City; F. C. NEWCOMBE of Ann Arbor; ANITA NEWCOMB MCGEE, of Washington; ARCHIBALD BLUE of Toronto; R. S. WOODWARD of New York.

From the Association at Large.—To hold over until successors are elected. A fellow elected from each section.—E. W. HYDE of Cincinnati (**A**); H. T. EDDY of Cincinnati (**B**); C. B. DUDLEY of Altoona (**C**); THOMAS GRAY of Terre Haute (**D**); W. N. RICE of Middletown (**E**); F. M. WEBSTER of Wooster (**F**); W. J. BEAL of Michigan Agricultural College (**G**); W. H. HOLMES of Washington (**H**); MARCUS BENJAMIN of Washington (**I**).

SPECIAL COMMITTEES OF THE ASSOCIATION.[1]

1. *Auditors.*

EMORY MCCLINTOCK, Morristown, and S. C. CHANDLER, Cambridge.

2. *Committee on Indexing Chemical Literature.*

H. CARRINGTON BOLTON, *Chairman*, F. W. CLARKE, A. R. LEEDS, H. W. WILEY, J. W. LANGLEY, A. B. PRESCOTT, ALFRED TUCKERMAN.

3. *Committee on the Association Table in Biological Laboratory at Wood's Holl.*

THE OUTGOING AND INCOMING VICE PRESIDENTS OF SECTIONS F and G, and C. O. WHITMAN, Chicago.

4. *Committee on the Policy of the Association.*

THE PRESIDENT, *Chairman*, THE PERMANENT SECRETARY, THE TREASURER, L. O. HOWARD, T. C. MENDENHALL, MANSFIELD MERRIMAN, WM. H. BREWER.

5. *Committee on Standards of Measurements.*

T. C. MENDENHALL, *Chairman*, W. A. ROGERS, E. L. NICHOLS, R. S. WOODWARD H. A. ROLAND, H. S. CARHART. With power to add to its number.

6. *Committee on Standard Colors and Standard Nomenclature of Colors.*

O. N. ROOD, *Chairman*, W. LECONTE STEVENS, WILLIAM HALLOCK.

7. *Committee on the Association Library.*

ALFRED SPRINGER, *Chairman*, A. W. BUTLER, W. L. DUDLEY, T. H. NORTON, THOS. FRENCH, JR.

8. *Committee for the study of the White Race in America.*

D. G. BRINTON, J. MC K. CATTELL, W. W. NEWELL, W J MCGEE, FRANZ BOAS.

9. *Committee to coöperate with the National Educational Association regarding the Teaching of Science in the Secondary Schools.*

R. S. TARR, H. S. CARHART, A. S. PACKARD, C. F. MABERRY, C. E. BESSEY.

10. *To represent the Association as Member of the American Advisory Board on an International Code of Zoölogical Nomenclature.*

A. S. PACKARD.

11. *Committee on Uniform Nomenclature in Scientific Work.*

R. T. COLBURN, *Chairman*, E. S. MORSE, A. B. PRESCOTT, R. S. WOODWARD, L. O. HOWARD.

12. *Committee on Extending the Influence of the Association into the Secondary Schools.*

E. S. MORSE, W. ORR, JR., T. C. CHAMBERLIN.

[1] All Committees are expected to present their reports to the COUNCIL not later than the third day of the meeting. Committees sending their reports to the Permanent Secretary one month before a meeting can have them printed for use at the meeting.

LOCAL COMMITTEES

FOR

DETROIT MEETING.

For a full list of Members of the several Local Committees see General and Daily Programmes of the Meeting.

HONORARY COMMITTEE.

General RUSSELL A. ALGER, *Chairman.*
JOHN A. RUSSELL, *Secretary.*

COMMITTEE ON INVITATION.

Mayor WILLIAM C. MAYBURY, *Chairman.*
JOHN A. RUSSELL, *Secretary.*

GENERAL COMMITTEE.

DEXTER M. FERRY, *Chairman.*
THOMAS W. PALMER, *Vice-Chairman.*
JOHN A. RUSSELL, *Secretary.*

EXECUTIVE COMMITTEE.

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DAVID E. HEINEMAN, *Secretary.*

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MISS CLARA A. AVERY, *Chairman.*
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BLAINE GAVETT, *Secretary.*

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E. W. PENDLETON, *Chairman.*
EARL D. BABST, *Secretary.*

COMMITTEE ON PLACE OF MEETING.

FREDERICK L. BLISS, *Chairman.*

COMMITTEE ON EXHIBITS.

GILBERT WILKES, *Chairman.*

COMMITTEE ON EXCURSIONS AND ENTERTAINMENTS.

FRED E. FARNSWORTH, *Chairman.*
A. H. GRIFFITH, *Secretary.*

PRINTING COMMITTEE.

JOHN A. RUSSELL, *Chairman.*

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THEODORE E. QUINBY, *Chairman.*
MILTON CARMICHAEL, *Secretary.*

FINANCE COMMITTEE.

THOMAS W. PALMER, *Chairman.*
COLLINS B. HUBBARD, *Treasurer.*

AUDITING COMMITTEE.

WILLIAM S. CRANE, *Chairman.*

OFFICERS ELECTED

FOR THE

BOSTON MEETING.

FIFTIETH ANNIVERSARY.

PRESIDENT.

F. W. PUTNAM, Harvard University.

VICE PRESIDENTS.

A. **Mathematics and Astronomy** — E. E. BARNARD, Yerkes Observatory.
B. **Physics** — FRANK P. WHITMAN, Adelbert College.
C. **Chemistry** — EDGAR F. SMITH, University of Pennsylvania.
D. **Mechanical Science and Engineering** — M. E. COOLEY, University of Michigan.
E. **Geology and Geography** — H. L. FAIRCHILD, University of Rochester.
F. **Zoölogy** — A. S. PACKARD, Brown University.
G. **Botany** — W. G. FARLOW, Harvard University.
H. **Anthropology** — J. McK. CATTELL, Columbia University.
I. **Social and Economic Science** — ARCHIBALD BLUE, Bureau of Mines, Toronto.

PERMANENT SECRETARY.

L. O. HOWARD, U. S. Department of Agriculture.

GENERAL SECRETARY.

DAVID S. KELLICOTT, Ohio State University. (*Deceased.*)
JAMES MCMAHON, Cornell University elected to fill vacancy.

SECRETARY OF THE COUNCIL.

FREDERICK BEDELL, Cornell University.

SECRETARIES OF THE SECTIONS.

A. **Mathematics and Astronomy** — ALEXANDER ZIWET, University of Michigan (*resigned*). JAMES MCMAHON, Cornell University, *Acting Secretary.*
B. **Physics** — E. B. ROSA, Wesleyan University (*resigned*). W. S. FRANKLIN, Lehigh University, *Acting Secretary.*
C. **Chemistry** — CHARLES BASKERVILLE, University of North Carolina.
D. **Mechanical Science and Engineering** — W. S. ALDRICH, University of West Virginia.
E. **Geology and Geography** — WARREN UPHAM, Minnesota Historical Society.
F. **Zoölogy** — C. W. STILES, U. S. Department of Agriculture (*resigned*). R. T. JACKSON, Harvard University, *Acting Secretary.*
G. **Botany** — ERWIN F. SMITH, U. S. Department of Agriculture.
H. **Anthropology** — M. H. SAVILLE, American Museum of Natural History.
I. **Social and Economic Science** — MARCUS BENJAMIN, U. S. National Museum.

TREASURER.

R. S. WOODWARD, Columbia University.

MEMBERS OF THE COUNCIL

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Officers of the Boston Meeting.— F. W. PUTNAM of Cambridge; E. E. BARNARD of Williams Bay; F. P. WHITMAN of Cleveland; E. F. SMITH of Philadelphia; M. E. COOLEY of Ann Arbor; H. L. FAIRCHILD of Rochester, A. S. PACKARD of Providence; W. G. FARLOW of Cambridge; J. MCK. CATTELL of New York; ARCHIBALD BLUE of Toronto, L. O. HOWARD of Washington; JAMES MCMAHON of Ithaca; FREDERICK BEDELL of Ithaca; JAMES MCMAHON of Ithaca, *Acting Secretary* of A; W. S. FRANKLIN of S. Bethlehem, *Acting Secretary* of B; CHARLES BASKERVILLE of Chapel Hill; W. S. ALDRICH of Morgantown; WARREN UPHAM of St. Paul; R. T. JACKSON of Cambridge, *Acting Secretary* of F; E. F. SMITH of Washington; M. H. SAVILLE of New York; MARCUS BENJAMIN of Washington; R. S. WOODWARD of New York.

From the Association at Large.— To hold over until successors are elected. A fellow elected from each section.— E. W. HYDE of Cincinnati (A); H. T. EDDY of Cincinnati (B); C. B. DUDLEY of Altoona (C); THOMAS GRAY of Terre Haute (D); W. N. RICE of Middletown (E); F. M. WEBSTER of Wooster (F); W. J. BEAL of Agricultural College, Michigan (G); W. H. HOLMES of Washington (H); MARCUS BENJAMIN of Washington (I).

(12)

MEETINGS AND OFFICERS OF THE ASSOCIATION OF AMERICAN GEOLOGISTS AND NATURALISTS.

MEETING.	DATE.	PLACE.	CHAIRMAN.	SECRETARY.	ASSIS'T SEC'Y.	TREASURER.
1st	April 2, 1840,	Philadelphia,	Edward Hitchcock,*	L. C. Beck,*		
2d	April 5, 1841,	Philadelphia,	Benjamin Silliman,*	L. C. Beck,*	B. Silliman, jr.,* C. B. Trego,* J. D. Whitney,* M. B. Williams,*	
3d	April 25, 1842,	Boston,	S. G. Morton,*	C. T. Jackson,*		
4th	April 26, 1843,	Albany,	Henry D. Rogers,*	B. Silliman, jr.,*		John Locke.*
5th	May 8, 1844,	Washington,	John Locke,*	B. Silliman, jr.,* O. P. Hubbard,		Douglas Houghton.*
6th	April 30, 1845,	New Haven,	Wm. B. Rogers,*	B. Silliman, jr.,* J. Lawrence Smith,*		Douglas Houghton.*
7th	Sept. 2, 1846,	New York,	C. T. Jackson,*	B. Silliman, jr.,*		E. C. Herrick.*
8th	Sept. 20, 1847,	Boston,	Wm. B. Rogers,†*	Jeffries Wyman,*		B. Silliman, jr.*

* Deceased.

† Professor ROGERS, as chairman of this last meeting, called the first meeting of the new Association to order and presided until it was fully organized by the adoption of a constitution. As he was thus the first presiding officer of the new Association, it was directed at the Hartford meeting that his name be placed at the head of the Past Presidents of the American Association for the Advancement of Science.

MEETINGS.	PLACE.	DATE.	MEMBERS IN ATTENDANCE.	NUMBER OF MEMBERS.
1.	Philadelphia	Sept. 20, 1848	?	461
2.	Cambridge	Aug. 14, 1849	?	540
3.	Charleston	Mar. 12, 1850	?	622
4.	New Haven	Aug. 19, 1850	?	704
5.	Cincinnati	May 5, 1851	87	800
6.	Albany	Aug. 19, 1851	194	769
7.	Cleveland	July 28, 1853	?	940
8.	Washington	April 26, 1854	168	1004
9.	Providence	Aug. 15, 1855	166	605
10.	2nd Albany	Aug. 20, 1856	381	722
11.	Montreal	Aug. 12, 1857	351	946
12.	Baltimore	April 28, 1858	190	962
13.	Springfield	Aug. 3, 1859	190	862
14.	Newport	Aug. 1, 1860	135	644
15.	Buffalo	Aug. 15, 1866	79	687
16.	Burlington	Aug. 21, 1867	73	415
17.	Chicago	Aug. 5, 1868	259	686
18.	Salem	Aug. 18, 1869	244	511
19.	Troy	Aug. 17, 1870	188	536
20.	Indianapolis	Aug. 16, 1871	196	668
21.	Dubuque	Aug. 15, 1872	164	610
22.	Portland	Aug. 20, 1873	195	670
23.	Hartford	Aug. 12, 1874	224	722
24.	Detroit	Aug. 11, 1875	165	807
25.	2nd Buffalo	Aug. 23, 1876	215	867
26.	Nashville	Aug. 29, 1877	173	958
27.	St. Louis	Aug. 21, 1878	134	962
28.	Saratoga	Aug. 27, 1879	256	1030
29.	Boston	Aug. 25, 1880	997	1555
30.	2nd Cincinnati	Aug. 17, 1881	500	1699
31.	2nd Montreal	Aug. 23, 1882	937	1922
32.	Minneapolis	Aug. 15, 1883	328	2033
33.	2nd Philadelphia	Sept. 3, 1884	1261*	1981
34.	Ann Arbor	Aug. 26, 1885	364	1956
35.	3d Buffalo	Aug. 18, 1886	445	1886
36.	New York	Aug. 10, 1887	729	1956
37.	2nd Cleveland	Aug. 14, 1888	342	1964
38.	Toronto	Aug. 26, 1889	424	1952
39.	2d Indianapolis	Aug. 19, 1890	364	1944
40.	2d Washington	Aug. 19, 1891	653†	2054
41.	Rochester	Aug. 17, 1892	456	2037
42.	Madison	Aug. 17, 1893	290	1939
43.	Brooklyn	Aug. 15, 1894	488	1802
44.	2d Springfield	Aug. 28, 1895	368	1913
45.	4th Buffalo	Aug. 24, 1896	333	1802
46.	2d Detroit	Aug. 9, 1897	283‡	1610

* Including 303 members of the British Association and 9 other foreign guests.
† Including 24 Foreign Honorary members for the meeting.
‡ Including 15 Foreign Honorary members and associates for the meeting.

OFFICERS OF THE MEETINGS OF THE ASSOCIATION.

[The number before the name is that of the meeting; the year of the meeting follows the name; the asterisk after a name indicates that the member is deceased.]

PRESIDENTS.

1. { WM. B. ROGERS,* 1848.
   { W. C. REDFIELD,* 1848.

2. JOSEPH HENRY,* 1849.

3, 4, 5. A. D. BACHE,* March meeting, 1850, in absence of JOSEPH HENRY.* August meeting, 1850. May meeting, 1851.

6. LOUIS AGASSIZ,* August meeting, 1851.

(No meeting in 1852).

7. BENJAMIN PIERCE,* 1853.

8. JAMES D. DANA,* 1854.

9. JOHN TORREY,* 1855.

10. JAMES HALL, 1856.

11, 12. ALEXIS CASWELL,* 1857, in place of J. W. BAILEY,* deceased. 1858, in absence of JEFFRIES WYMAN.*

13. STEPHEN ALEXANDER,* 1859.

14. ISAAC LEA,* 1860.

(No meetings for 1861–65).

15. F. A. P. BARNARD,* 1866.

16. J. S. NEWBERRY,* 1867.

17. B. A. GOULD,* 1868.

18. J. W. FOSTER,* 1869.

19. T. STERRY HUNT,* 1870, in the absence of WM. CHAUVENET.*

20. ASA GRAY,* 1871.

21. J. LAWRENCE SMITH,* 1872.

22. JOSEPH LOVERING,* 1873.

23. J. L. LECONTE,* 1874.

24. J. E. HILGARD,* 1875.

25. WILLIAM B. ROGERS,* 1876.

26. SIMON NEWCOMB, 1877.

27. O. C. MARSH, 1878.

28. G. F. BARKER, 1879.

29. LEWIS H. MORGAN,* 1880.

30. G. J. BRUSH, 1881.

31. J. W. DAWSON, 1882.

32. C. A. YOUNG, 1883.

33. J. P. LESLEY, 1884.

34. H. A. NEWTON,* 1885.

35. EDWARD S. MORSE, 1886.

36. S. P. LANGLEY, 1887.

37. J. W. POWELL, 1888.

38. T. C. MENDENHALL, 1889.

39. G. LINCOLN GOODALE, 1890.

40. ALBERT B. PRESCOTT, 1891.

41. JOSEPH LECONTE, 1892.

42. WILLIAM HARKNESS, 1893.

43. DANIEL G. BRINTON, 1894.

44. E. W. MORLEY, 1895.

45. EDWARD D. COPE,* 1896.

46. WOLCOTT GIBBS, 1897, absent. W J MCGEE, Acting President.

47. F. W. PUTNAM, 1898.

VICE PRESIDENTS.

There were no Vice Presidents until the 11th meeting when there was a single Vice President for each meeting. At the 24th meeting the Association met in Sections A and B, each presided over by a Vice President. At the 31st meeting nine sections were organized, each with a Vice President as its presiding officer. In 1886, Section G (Microscopy) was given up. In 1892, Section F was divided into F, Zoology; G, Botany.

1857–1874.

11. ALEXIS CASWELL,* 1857, acted as President.
12. JOHN E. HOLBROOK,* 1858, not present.
13. EDWARD HITCHCOCK,* 1859.
14. B. A. GOULD,* 1860.
15. A. A. GOULD,* 1866, in absence of R. W. GIBBES.
16. WOLCOTT GIBBS, 1867.
17. CHARLES WHITTLESEY,* 1868.
18. OGDEN N. ROOD, 1869.
19. T. STERRY HUNT,* 1870, acted as President.
20. G. F. BARKER, 1871.
21. ALEXANDER WINCHELL,* 1872.
22. A. H. WORTHEN,* 1873, not present.
23. C. S. LYMAN,* 1874.

1875–1881.

Section A.—Mathematics, Physics and Chemistry.

24. H. A. NEWTON,* 1875.
25. C. A. YOUNG, 1876.
26. R. H. THURSTON, 1877, in the absence of E. C. PICKERING.
27. R. H. THURSTON, 1878.
28. S. P. LANGLEY, 1879.
29. ASAPH HALL, 1880.
30. WILLIAM HARKNESS, 1881, in the absence of A. M. MAYER.*

Section B.—Natural History.

24. J. W. DAWSON, 1875.
25. EDWARD S. MORSE, 1876.
26. O. C. MARSH, 1877.
27. AUG. R. GROTE, 1878.
28. J. W. POWELL, 1879.
29. ALEXANDER AGASSIZ, 1880.
30. EDWARD T. COX, 1881, in the absence of GEORGE ENGELMANN.*

CHAIRMEN OF SUBSECTIONS, 1875–1881.

Subsection of Chemistry.

24. S. W. JOHNSON, 1875.
25. G. F. BARKER, 1876.
26. N. T. LUPTON,* 1877.
27. F. W. CLARKE, 1878.
28. F. W. CLARKE, 1879, in the absence of IRA REMSEN.
29. J. M. ORDWAY, 1880.
30. G. C. CALDWELL, 1881, in the absence of W. R. NICHOLS.*

Subsection of Microscopy.

25. R. H. WARD, 1876.
26. R. H. WARD, 1877.
27. R. H. WARD, 1878, in the absence of G. S. BLACKIE.*

28. E. W. MORLEY, 1879.
29. S. A. LATTIMORE, 1880.
30. A. B. HERVEY, 1881.

Subsection of Anthropology.

24. LEWIS H. MORGAN,* 1875.
25. LEWIS H. MORGAN,* 1876.
26. DANIEL WILSON,* 1877, not present.
27. United with Section B.
28. DANIEL WILSON,* 1879.
29. J. W. POWELL, 1880.
30. GARRICK MALLERY,* 1881.

Subsection of Entomology.

30. J. G. MORRIS,* 1881.

VICE PRESIDENTS OF SECTIONS, 1882–

Section A.—Mathematics and Astronomy.

31. W. A. ROGERS, 1882, in the absence of WM. HARKNESS.
32. W. A. ROGERS, 1883.
33. H. T. EDDY, 1884.
34. WM. HARKNESS, 1885, in the absence of J. M. VAN VLECK.
35. J. W. GIBBS, 1886.
36. J. R. EASTMAN, 1887, in place of W. FERREL.* resigned.
37. ORMOND STONE, 1888.
38. R. S. WOODWARD, 1889.
39. S. C. CHANDLER, 1890.
40. E. W. HYDE, 1891.
41. J. R. EASTMAN, 1892.
42. C. L. DOOLITTLE, 1893.
43. { G. C. COMSTOCK, 1894. EDGAR FRISBY, 1894.
44. EDGAR FRISBY, 1895, in place of E. H. HOLDEN, resigned.
45. ALEX. MACFARLANE, 1896 in place of WM. E. STORY, resigned.
46. W. W. BEMAN, 1897.
47. E. E. BARNARD, 1898.

Section B.—Physics.

31. T. C. MENDENHALL, 1882.
32. H. A. ROWLAND, 1883
33. J. TROWBRIDGE, 1884.
34. S. P. LANGLEY, 1885, in place of C. F. BRACKETT, resigned.
35. C. F. BRACKETT, 1886.
36. W. A. ANTHONY, 1887.
37. A. A. MICHELSON, 1888.
38. H. S. CARHART, 1889.
39. CLEVELAND ABBE, 1890.
40. F. E. NIPHER, 1891.
41. B. F. THOMAS, 1892.
42. E. L. NICHOLS, 1893.
43. WM. A. ROGERS, 1894.
44. W. LECONTE STEVENS, 1895.
45. CARL LEO MEES, 1896.
46. CARL BARUS, 1897.
47. F. P. WHITMAN, 1898.

Section C.—Chemistry.

31. H. C. BOLTON, 1882.
32. E. W. MORLEY, 1883.
33. J. W. LANGLEY, 1884.
34. N. T. LUPTON,* 1885, in absence of W. R. NICHOLS.*
35. H. W. WILEY, 1886.
36. A. B. PRESCOTT, 1887.
37. C. E. MUNROE, 1888.
38. W. L. DUDLEY, 1889.
39. R. B. WARDER, 1890.
40. R. C. KEDZIE, 1891.
41. ALFRED SPRINGER, 1892.
42. EDWARD HART, 1893.
43. T. H. NORTON, 1894.
44. WM. MCMURTRIE, 1895.
45. W. A. NOYES, 1896.
46. W. P. MASON, 1897.
47. EDGAR F. SMITH, 1898.

Section D.—Mechanical Science and Engineering.

31. W. P. TROWBRIDGE,* 1882.
32. DE VOLSON WOOD, 1883, absent, but place was not filled.
33. R. H. THURSTON, 1884.
34. J. BURKITT WEBB, 1885.
35. O. CHANUTE, 1886.
36. E. B. COXE, 1887.
37. C. J. H. WOODBURY, 1888.
38. JAMES E. DENTON, 1889.
39. JAMES E. DENTON, 1890, in place of A. BEARDSLEY, absent.
40. THOMAS GRAY, 1891.
41. J. B. JOHNSON, 1892.
42. S. W. ROBINSON, 1893.
43. MANSFIELD MERRIMAN, 1894.
44. WILLIAM KENT, 1895.
45. FRANK O. MARVIN, 1896.
46. JOHN GALBRAITH, 1897.
47. M. E. COOLEY, 1898.

VICE PRESIDENTS OF SECTIONS, CONTINUED.

Section E.—Geology and Geography.

31. E. T. Cox, 1882.
32. C. H. Hitchcock, 1883.
33. N. H. Winchell, 1884.
34. Edward Orton, 1885.
35. T. C. Chamberlin, 1886.
36. G. K. Gilbert, 1887.
37. George H. Cook,* 1888.
38. Charles A. White, 1889.
39. John C. Branner, 1890.
40. J. J. Stevenson, 1891.
41. H. S. Williams, 1892.
42. Charles D. Walcott, 1893.
43. Samuel Calvin, 1894.
44. Jed. Hotchkiss, 1895.
45. B. K. Emerson, 1896.
46. { I. C. White, 1897. E. W. Claypole, 1897. }
47. H. L. Fairchild, 1898.

Section F.—Biology, 1882-92.

31. W. H. Dall, 1882.
32. W. J. Beal, 1883.
33. E. D. Cope,* 1884.
34. T. J. Burrill, 1885, in the absence of B. G. Wilder.
35. H. P. Bowditch, 1886.
36. W. G. Farlow, 1887.
37. C. V. Riley,* 1888.
38. George L. Goodale, 1889.
39. C. S. Minot, 1890.
40. J. M. Coulter, 1891.
41. S. H. Gage, 1892.

Section F.— Zoölogy.

42. Henry F. Osborn, 1893.
43. J. A. Lintner, 1894, in place of S. H. Scudder, resigned.
44. L. O. Howard, 1895, in place of D. S. Jordan, resigned.
45. Theo. Gill, 1896.
46. L. O. Howard, 1897, in place of G. Brown Goode,* deceased.
47. A. S. Packard, 1898.

Section G.—Microscopy, 1882-85.

31. A. H. Tuttle, 1882.
32. J. D. Cox, 1883.
33. T. G. Wormley, 1884.
34. S. H. Gage, 1885.

(Section united with F in 1886.)

Section G.—Botany.

42. Charles E. Bessey, 1893.
43. { L. M. Underwood, 1894. C. E. Bessey, 1894. }
44. J. C. Arthur, 1895.
45. N. L. Britton, 1896.
46. G. F. Atkinson, 1897.
47. W. G. Farlow, 1898.

Section H.—Anthropology.

31. Alexander Winchell,* 1882.
32. Otis T. Mason, 1883.
33. Edward S. Morse, 1884.
34. J. Owen Dorsey,* 1885, in absence of W. H. Dall.
35. Horatio Hale,* 1886.
36. D. G. Brinton, 1887.
37. Charles C. Abbott, 1888.
38. Garrick Mallery,* 1889.
39. Frank Baker, 1890.
40. Joseph Jastrow, 1891.
41. W. H. Holmes, 1892.
42. J. Owen Dorsey,* 1893.
43. Franz Boas, 1894.
44. F. H. Cushing, 1895.
45. Alice C. Fletcher, 1896.
46. W J McGee, 1897.
47. J. McK. Cattell, 1898.

Section I. — Social and Economic Science.

31. E. B. Elliott,* 1882.
32. Franklin B. Hough,* 1883.
33. John Eaton,* 1884.
34. Edward Atkinson, 1885.
35. Joseph Cummings,* 1886.
36. H. E. Alvord, 1887.
37. Charles W. Smiley, 1888.
38. Charles S. Hill, 1889.
39. J. Richards Dodge, 1890.
40. Edmund J. James, 1891.
41. L. F. Ward, 1892, in place of S. D. Horton,* resigned.
42. William H. Brewer, 1893.
43. Henry Farquhar, 1894.
44. B. E. Fernow, 1895.
45. W. L. Lazenby, 1896.
46. R. T. Colburn, 1897.
47. Archibald Blue, 1898.

SECRETARIES.

General Secretaries, 1848–

1. WALTER R. JOHNSON,* 1848.
2. E. N. HORSFORD,* 1849, in absence of JEFFRIES WYMAN.*
3. L. R. GIBBS, 1850, in absence of E. C. HERRICK.*
4. E. C. HERRICK,* 1850.
5. WILLIAM B. ROGERS,* 1851, in absence of E. C. HERRICK.*
6. WILLIAM B. ROGERS,* 1851.
7. S. ST. JOHN,* 1853, in absence of J. D. DANA.*
8. J. LAWRENCE SMITH,* 1854.
9. WOLCOTT GIBBS, 1855.
10. B. A. GOULD,* 1856.
11. JOHN LECONTE,* 1857.
12. W. M. GILLESPIE,* 1858, in absence of WM. CHAUVENET.*
13. WILLIAM CHAUVENET,* 1859.
14. JOSEPH LECONTE, 1860.
15. ELIAS LOOMIS,* 1866, in the absence of W. P. TROWBRIDGE*
16. C. S LYMAN,* 1867.
17. SIMON NEWCOMB, 1868, in absence of A. P. ROCKWELL.
18. O. C. MARSH, 1869.
19. F. W. PUTNAM, 1870, in absence of C. F. HARTT.*
20. F. W. PUTNAM, 1871.
21. EDWARD S. MORSE, 1872.
22. C. A. WHITE, 1873.
23. A. C. HAMLIN, 1874.
24. S. H. SCUDDER, 1875.
25. T. C. MENDENHALL, 1876.
26. AUG. R. GROTE, 1877.
27. H. C. BOLTON, 1878.
28. H. C. BOLTON, 1879, in the absence of GEORGE LITTLE.
29. J. K. REES. 1880.
30. C. V. RILEY,* 1881.
31. WILLIAM SAUNDERS, 1882.
32. J. R. EASTMAN, 1883.
33. ALFRED SPRINGER, 1884.
34. C. S. MINOT, 1885.
35. S. G WILLIAMS, 1886.
36. WILLIAM H. PETTEE, 1887.
37. JULIUS POHLMAN, 1888.
38. C. LEO MEES, 1889.
39. H. C. BOLTON, 1890.
40. H. W. WILEY, 1891.
41. A. W. BUTLER, 1892.
42. T. H. NORTON, 1893.
43. H. L. FAIRCHILD, 1894.
44. JAS. LEWIS HOWE, 1895.
45. CHARLES R. BARNES, 1896.
46. ASAPH HALL, JR., 1897.
47. D. S. KELLICOTT, 1898.

Permanent Secretaries, 1851–

5–7. SPENCER F. BAIRD,* 1851–4.
8–17. JOSEPH LOVERING,* 1854–68.
18. F. W. PUTNAM, 1869, in the absence of J. LOVERING.*
19–21. JOSEPH LOVERING,* 1870–73.
22–46. F. W. PUTNAM, 1873–98.
47–48. L. O. HOWARD. 1898–99.

Assistant General Secretaries, 1882-1887.

31. J. R. EASTMAN, 1882.
32. ALFRED SPRINGER, 1883.
33. C. S. MINOT, 1884, in absence of E. S. HOLDEN.
34. S. G. WILLIAMS, 1885, in the absence of C. C. ABBOTT.
35. W. H. PETTEE, 1886.
36. J. C. ARTHUR, 1887.

Secretaries of the Council, 1888–

37. C. LEO MEES, 1888.
38. H. C. BOLTON, 1889.
39. H. W. WILEY, 1890.
40. A. W. BUTLER, 1891.
41. T. H. NORTON, 1892.
42. H. LEROY FAIRCHILD, 1893.
43. JAS. LEWIS HOWE, 1894.
44. CHARLES R. BARNES, 1895.
45. ASAPH HALL, JR., 1896.
46. D. S. KELLICOTT, 1897.
47. FREDERICK BEDELL, 1898.

Secretaries of Section A.—Mathematics, Physics and Chemistry, 1875–81.

24. { S. P. LANGLEY, 1875. T. C. MENDENHALL, 1875. }
25. A. W. WRIGHT, 1876.
26. H. C. BOLTON, 1877.
27. F. E. NIPHER, 1878.
28. J. K. REES, 1879.
29. H. B. MASON, 1880.
30. E. T. TAPPAN, 1881, in the absence of JOHN TROWBRIDGE.

Secretaries of Section B.—Natural History, 1875–81.

24. EDWARD S. MORSE, 1875.
25. ALBERT H. TUTTLE, 1876.
26. WILLIAM H. DALL, 1877.
27. GEORGE LITTLE, 1878.
28. WILLIAM H. DALL, 1879, in the absence of A. C. WETHERBY.
29. CHARLES V. RILEY,* 1880.
30. WILLIAM SAUNDERS, 1881.

SECRETARIES OF SUBSECTIONS, 1875–81.

Subsection of Chemistry.

24. F. W. CLARKE, 1875.
25. H. C. BOLTON, 1876.
26. P. SCHWEITZER, 1877.
27. A. P. S. STUART, 1878.
28. W. R. NICHOLS,* 1879.
29. C. E. MUNROE, 1880.
30. ALFRED SPRINGER, 1881, in the absence of R. B. WARDER.

Subsection of Entomology.

30. B P. MANN, 1881.

Subsection of Anthropology.

24. F. W. PUTNAM, 1875.
25. OTIS T. MASON, 1876.
26, 27. United with Section B.
28, 29, 30. J. G. HENDERSON. 1879–81.

Subsection of Microscopy.

25. E. W. MORLEY, 1876.
26. T. O. SOMMERS, JR., 1877.
27. G. J. ENGELMANN, 1878.
28, 29. A. B. HERVEY, 1879–1880.
30. W. H. SEAMAN, 1881, in the absence of S. P. SHARPLES.

SECRETARIES OF THE SECTIONS, 1882–

Section A.—Mathematics and Astronomy.

31. H. T. EDDY, 1882.
32. G. W. HOUGH, 1883, in the absence of W. W. JOHNSON.
33. G. W. HOUGH, 1884.
34. E. W. HYDE, 1885.
35. S. C. CHANDLER, 1886.
36. H. M. PAUL, 1887.
37. C. C. DOOLITTLE, 1888.
38. G. C. COMSTOCK, 1889.
39. W. W. BEMAN, 1890.
40. F. H. BIGELOW, 1891.
41. WINSLOW UPTON, 1892.
42. C. A. WALDO, 1893, in the absence of A. W. PHILLIPS.
43. J. C. KERSHNER, 1894, in place of W. W. BEMAN, resigned.
44. ASAPH HALL, JR., 1895, in place of E. H. MOORE, resigned.
45. EDWIN B. FROST, 1896.
46. JAMES MCMAHON, 1897.
47. —— ——— 1898, in place of ALEXANDER ZIWET, resigned.

Section B.—Physics.

31. C. S. HASTINGS, 1882.
32. F. E. NIPHER, 1883, in the absence of C. K. WEAD.
33. N. D. C. HODGES, 1884.
34. B. F. THOMAS, 1885, in place of A. A. MICHELSON, resigned.
35. H. S. CARHART, 1886.
36. C. LEO MEES, 1887.
37. ALEX. MACFARLANE, 1888.
38. E. L. NICHOLS, 1889.
39. E. M. AVERY, 1890.
40. ALEX. MACFARLANE, 1891.
41. BROWN AYRES, 1892.
42. W. LECONTE STEVENS, 1893.
43. B. W. SNOW, 1894.
44. E. MERRITT, 1895.
45. FRANK P. WHITMAN, 1896.
46. FREDERICK BEDELL, 1897.
47. —— ———, 1898, in place of E. B. ROSA, resigned.

SECRETARIES OF THE SECTIONS, CONTINUED.

Section C.—Chemistry.

31. ALFRED SPRINGER, 1882.
32. J. W. LANGLEY, 1883. W. MCMURTRIE, 1883.
33. H. CARMICHAEL, 1884, in the absence of R. B. WARDER.
34. F. P. DUNNINGTON, 1885.
35. W. MCMURTRIE, 1886.
36. C. S. MABERY, 1887.
37. W. L. DUDLEY, 1888.
38. EDWARD HART, 1889.
39. W. A. NOYES, 1890.
40. T. H. NORTON, 1891.
41. JAS. LEWIS HOWE, 1892.
42. H. N. STOKES, 1893, in the absence of J. U. NEF.
43. MORRIS LOEB, 1894, in place of S. M. BABCOCK, resigned.
44. W. P. MASON, 1895. W. O. ATWATER, 1895.
45. FRANK P. VENABLE, 1896.
46. P. C. FREER, 1897.
47. C. BASKERVILLE, 1898.

Section D.—Mechanical Science and Engineering.

31. J. BURKITT WEBB, 1882, in the absence of C. R. DUDLEY.
32. J. BURKITT WEBB, 1883, *pro tempore.*
33. J. BURKITT WEBB, 1884.
34. C. J. H. WOODBURY, 1885.
35. WILLIAM KENT, 1886.
36. G. M. BOND, 1887.
37. ARTHUR BEARDSLEY, 1888.
38. W. B. WARNER, 1889.
39. THOMAS GRAY, 1890.
40. WILLIAM KENT, 1891.
41. O. H. LANDRETH, 1892.
42. D. S. JACOBUS, 1893.
43. JOHN H. KINEALY, 1894.
44. H. S. JACOBY, 1895.
45. JOHN GALBRAITH, 1896.
46. JOHN J. FLATHER, 1897.
47. W. S. ALDRICH, 1898.

Section E.—Geology and Geography.

31. H. S. WILLIAMS, 1882, in the absence of C. E. DUTTON.
32. A. A. JULIEN, 1883.
33. E. A. SMITH, 1884.
34. G. K. GILBERT, 1885, in the absence of H. C. LEWIS.*
35. E. W. CLAYPOLE, 1886.
36. W. M. DAVIS, 1887, in the absence of T. B. COMSTOCK.
37. JOHN C. BRANNER, 1888.
38. JOHN C. BRANNER, 1889.
39. SAMUEL CALVIN, 1890.
40. W J MCGEE, 1891.
41. R. D. SALISBURY, 1892.
42. W. H. HOBBS, 1893, in place of R. T. HILL, resigned.
43. JED. HOTCHKISS, 1894, in place of W. M. DAVIS, resigned.
44. J. PERRIN SMITH, 1895.
45. W. N. RICE, 1896, in place of A. C. GILL, resigned.
46. C. H. SMYTH, JR., 1897.
47. WARREN UPHAM, 1898.

Section F.—Biology, 1882–92.

31. WILLIAM OSLER, 1882, in the absence of C. S. MINOT.
32. S. A. FORBES, 1883.
33. C. E. BESSEY, 1884.
34. J. A. LINTNER, 1885, in place of C. H. FERNALD, resigned.
35. J. C. ARTHUR, 1886.
36. J. H. COMSTOCK, 1887.
37. B. H. FERNOW, 1888.
38. A. W. BUTLER, 1889.
39. J. M. COULTER, 1890.
40. A. J. COOK, 1891.
41. B. D. HALSTED, 1892.

Section F.—Zoölogy.

42. L. O. HOWARD, 1893.
43. JOHN B. SMITH, 1894, in place of WM. LIBBY, JR., resigned.
44. C. W. HARGITT, 1895, in place of S. A. FORBES, resigned.
45. D. S. KELLICOTT, 1896.
46. C. C. NUTTING, 1897.
47. ——, 1898, in place of C. W. STILES, resigned.

Secretaries of the Sections, continued.

Section G.—Microscopy, 1882–85.

31. Robert Brown, jr. 1882.
32. Carl Seiler, 1883.
33. Romyn Hitchcock, 1884.
34. W. H. Walmsley, 1885.

Section G.—Botany.

42. B. T. Galloway, 1893, in the absence of F. V. Coville.
43. Charles R. Barnes, 1894.
44. B. T. Galloway, 1895. M. B. Waite, 1895.
45. George F. Atkinson, 1896.
46. F. C. Newcombe, 1897.
47. Erwin F. Smith, 1898.

Section H.—Anthropology.

31. Otis T. Mason, 1882.
32. G. H. Perkins, 1883.
33. G. H. Perkins, 1884, in the absence of W. H. Holmes.
34. Erminnie A. Smith,* 1885.
35. A. W. Butler, 1886.
36. Charles C. Abbott, 1887, in absence of F. W. Langdon.
37. Frank Baker, 1888.
38. W. M. Beauchamp, 1889.
39. Joseph Jastrow, 1890.
40. W. H. Holmes, 1891.
41. W. M. Beauchamp, 1892, in place of S. Culin, resigned.
42. Warren K. Moorehead, 1893.
43. A. F. Chamberlin, 1894.
44. Stewart Culin and W. W. Tooker, 1895, in place of Anita N. McGee resigned.
45. G. H. Perkins, 1896, in place of J. G. Bourke,* deceased.
46. Anita N. McGee, 1897, in place of Harlan I. Smith, resigned.
47. Marshall H. Saville, 1898.

Section I.—Social and Economic Science.

31. Franklin B. Hough,* 1882. J. Richards Dodge, 1882.
32. Joseph Cummings,* 1883.
33. Charles W. Smiley, 1884.
34. Charles W. Smiley, 1885, in absence of J. W. Chickering.
35. H. E. Alvord, 1886.
36. W. R. Lazenby, 1887.
37. Charles S. Hill, 1888.
38. J. Richards Dodge, 1889.
39. B. E. Fernow, 1890.
40. B. E. Fernow, 1891.
41. Henry Farquhar, 1892, in place of L. F. Ward made Vice-president.
42. Nellie S. Kedzie, 1893.
43. Manley Miles, 1894.
44. W. R. Lazenby, 1895, in place of E. A. Ross, resigned.
45. R. T. Colburn, 1896.
46. Archibald Blue, 1897.
47. Marcus Benjamin, 1898.

TREASURERS.

1. Jeffries Wyman,* 1848.
2. A. L. Elwyn,* 1849.
3. St. J. Ravenel,* 1850, in the absence of A. L. Elwyn.*
4. A. L. Elwyn,* 1850.
5. Spencer F. Baird,* 1851, in absence of A. L. Elwyn.*

6–7. A. L. Elwyn,* 1851–1853.

8. J. L. LeConte,* 1854, in absence of A. L. Elwyn.*

9–19. A. L. Elwyn,* 1855–1870.

20–30. William S. Vaux,* 1871–1881.

32–42. William Lilly,* 1882–1893.

43–47. R. S. Woodward, 1894–98.

COMMONWEALTH OF MASSACHUSETTS.

IN THE YEAR ONE THOUSAND EIGHT HUNDRED AND SEVENTY-FOUR.

AN ACT

To Incorporate the "American Association for the Advancement of Science."

Be it enacted by the Senate and House of Representatives, in General Court assembled, and by the authority of the same, as follows:

Section 1. Joseph Henry of Washington, Benjamin Pierce of Cambridge, James D. Dana of New Haven, James Hall of Albany, Alexis Caswell of Providence, Stephen Alexander of Princeton, Isaac Lea of Philadelphia, F. A P. Barnard of New York, John S. Newberry of Cleveland, B. A. Gould of Cambridge, T. Sterry Hunt of Boston, Asa Gray of Cambridge, J. Lawrence Smith of Louisville, Joseph Lovering of Cambridge and John LeConte of Philadelphia, their associates, the officers and members of the Association, known as the "American Association for the Advancement of Science," and their successors, are hereby made a corporation by the name of the "American Association for the Advancement of Science," for the purpose of receiving, purchasing, holding and conveying real and personal property, which it now is, or hereafter may be, possessed of, with all the powers and privileges, and subject to the restrictions, duties and liabilities set forth in the general laws which now or hereafter may be in force and applicable to such corporations.

Section 2. Said corporation may have and hold by purchase, grant, gift or otherwise, real estate not exceeding one hundred thousand dollars in value, and personal estate of the value of two hundred and fifty thousand dollars.

Section 3. Any two of the corporators above named are hereby authorized to call the first meeting of the said corporation in the month of August next ensuing, by notice thereof "by mail," to each member of the said Association.

Section 4. This act shall take effect upon its passage.

House of Representatives, March 10, 1874.
Passed to be enacted,
John E. Sanford, *Speaker*

In Senate, March 17, 1874.
Passed to be enacted.
Geo. B. Loring, *President*.

March 19, 1874.
Approved,
W. B. Washburn.

Secretary's Department,
Boston, April 3, 1874.
A true copy, Attest:
David Pulsifer,
Deputy Secretary of the Commonwealth.

CONSTITUTION

OF THE

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

Incorporated by Act of the General Court of the Commonwealth of Massachusetts.

OBJECTS.

ARTICLE 1. The objects of the Association are, by periodical and migratory meetings, to promote intercourse between those who are cultivating science in different parts of America, to give a stronger and more general impulse and more systematic direction to scientific research, and to procure for the labors of scientific men increased facilities and a wider usefulness.

MEMBERS, FELLOWS, PATRONS AND HONORARY FELLOWS.

ART. 2. The Association shall consist of Members, Fellows, Patrons, Corresponding Members and Honorary Fellows.

ART. 3. Any person may become a Member of the Association upon recommendation in writing by two members or fellows, and election by the Council. Any incorporated scientific society or institution, or any public or incorporated library, may be enrolled as a member of the Association by vote of the Council by payment of the initiation fee; such society, institution or library may be represented by either the President, Curator, Director or Librarian presenting proper credentials at any meeting of the Association for which the assessment has been paid.

ART. 4. Fellows shall be elected by the Council from such of the members as are professionally engaged in science, or have by their labors aided in advancing science. The election of fellows shall be by ballot and a majority vote of the members of the Council at a designated meeting of the Council.

ART. 5. Any person paying to the Association the sum of one thousand dollars shall be classed as a Patron, and shall be entitled to all the privileges of a member and to all its publications.

ART. 6. Honorary Fellows of the Association, not exceeding three for each section, may be elected; the nominations to be made by the Council and approved by ballot in the respective sections before election by ballot in General Session. Honorary Fellows shall be entitled to all the privileges of Fellows and shall be exempt from all fees and assessments, and entitled to all publications of the Association issued after the date of their election. Corresponding Members shall consist of such scientists not residing in America as may be elected by the Council, and their number shall be limited to fifty. Corresponding Members shall be entitled to all the privileges of members and to the annual volumes of Proceedings published subsequent to their election.

ART. 7. The name of any member or fellow two years in arrears for annual dues shall be erased from the list of the Association, provided that two notices of indebtedness, at an interval of at least three months, shall have been given; and no such person shall be restored until he has paid his arrearages or has been reëlected. The Council shall have power to exclude from the Association any member or fellow, on satisfactory evidence that said member or fellow is an improper person to be connected with the Association, or has in the estimation of the Council made improper use of his membership or fellowship.

ART. 8. No member or fellow shall take part in the organization of, or hold office in, more than one section at any one meeting.

OFFICERS.

ART. 9. The officers of the Association shall be elected by ballot by the Nominating Committee from the fellows, and shall consist of a President, a Vice President from each section, a Permanent Secretary, a General Secretary, a Secretary of the Council, a Treasurer, and a Secretary of each Section; these, with the exception of the Permanent Secretary, shall be elected at each meeting for the following one and, with the exception of the Treasurer and the Permanent Secretary, shall not be reëligible for the next two meetings. The term of office of Permanent Secretary shall be five years.

ART. 10. The President, or, in his absence, the senior Vice President present, shall preside at all General Sessions of the Association and at all meetings of the Council. It shall also be the duty of the President to give an address at a General Session of the Association at the meeting following that over which he presided.

ART. 11. The Vice Presidents shall be chairmen of their respective

Sections, and of their Sectional Committees, and it shall be part of their duty to give an address, each before his own section, at such time as the council shall determine. The Vice Presidents may appoint temporary Chairmen to preside over the sessions of their sections, but shall not delegate their other duties. The Vice Presidents shall have seniority in order of their continuous membership in the Association.

ART. 12. The General Secretary shall be the Secretary of all General Sessions of the Association, and shall keep a record of the business of these sessions. He shall receive the records from the Secretaries of the Sections, which, after examination, he shall transmit with his own records to the Permanent Secretary within two weeks after the adjournment of the meeting.

ART. 13. The Secretary of the Council shall keep the records of the Council. He shall give to the Secretary of each Section the titles of papers assigned to it by the Council. He shall receive proposals for membership and bring them before the Council.

ART. 14. The Permanent Secretary shall be the executive officer of the Association under the direction of the Council. He shall attend to all business not specially referred to committees nor otherwise constitutionally provided for. He shall keep an account of all business that he has transacted for the Association, and make annually a general report for publication in the annual volume of Proceedings. He shall attend to the printing and distribution of the annual volume of Proceedings, and all other printing ordered by the Association. He shall issue a circular of information to members and fellows at least three months before each meeting, and shall, in connection with the Local Committee, make all necessary arrangements for the meetings of the Association. He shall provide the Secretaries of the Association with such books and stationery as may be required for their records and business, and shall provide members and fellows with such blank forms as may be required for facilitating the business of the Association. He shall collect all assessments and admission fees, and notify members and fellows of their election, and of any arrearages. He shall receive, and bring before the Council, the titles and abstracts of papers proposed to be read before the Association. He shall keep an account of all receipts and expenditures of the Association, and report the same annually at the first meeting of the Council, and shall pay over to the Treasurer such unexpended funds as the Council may direct. He shall receive and hold in trust for the

Association all books, pamphlets and manuscripts belonging to the Association, and allow the use of the same under the provisions of the Constitution and the orders of the Council. He shall receive all communications addressed to the Association during the intervals between meetings, and properly attend to the same. He shall at each meeting report the names of fellows and members who have died since the preceding meeting. He shall be allowed a salary which shall be determined by the Council, and may employ one or more clerks at such compensation as may be agreed upon by the Council.

ART. 15. The Treasurer shall invest the funds received by him in such securities as may be directed by the Council. He shall annually present to the Council an account of the funds in his charge. No expenditure of the principal in the hands of the Treasurer shall be made without a unanimous vote of the Council, and no expenditure of the income received by the Treasurer shall be made without a two-thirds vote of the Council. The Treasurer shall give bonds for the faithful performance of his duty in such manner and sum as the Council shall from time to time direct.

ART. 16. The Secretaries of the Sections shall keep the records of their respective sections, and, at the close of the meeting, give the same, including the records of subsections, to the General Secretary. They shall also be the Secretaries of the Sectional Committees. The Secretaries shall have seniority in order of their continuous membership in the Association.

ART. 17. In case of a vacancy in the office of the President, one of the Vice Presidents shall be elected by the Council as the President of the meeting. Vacancies in the offices of Vice President, Permanent Secretary, General Secretary, Secretary of the Council, and Treasurer, shall be filled by the Council by ballot. A vacancy in the office of Secretary of a Section shall be filled by nomination and election by ballot in the Section.

ART. 18. The Council shall consist of the past Presidents, and the Vice Presidents of the last meeting, together with the President, the Vice Presidents, the Permanent Secretary, the General Secretary, the Secretary of the Council, the Secretaries of the Sections, and the Treasurer of the current meeting, with the addition of one fellow elected from each Section by ballot on the first day of its meeting. The members present at any regularly called meeting of the Council, provided there are at least five,

shall form a quorum for the transaction of business. The Council shall meet on the day preceding each annual meeting of the Association, and arrange the programme for the first day of the sessions. The time and place of this first meeting shall be designated by the Permanent Secretary. Unless otherwise agreed upon, regular meetings of the Council shall be held in the Council room at 9 o'clock, A.M., on each day of the meeting of the Association. Special meetings of the Council may be called at any time by the President. The Council shall be the board of supervision of the Association, and no business shall be transacted by the Association that has not first been referred to, or originated with, the Council. The Council shall receive and assign papers to the respective sections; examine and, if necessary, exclude papers; decide which papers, discussions and other proceedings shall be published, and have the general direction of the publications of the Association; manage the financial affairs of the Association; arrange the business and programmes for General Sessions; suggest subjects for discussion, investigation or reports; elect members and fellows; and receive and act upon all invitations extended to the Association and report the same at a General Session of the Association. The Council shall receive all reports of Special Committees and decide upon them, and only such shall be read in General Session as the Council shall direct. The Council shall appoint at each meeting the following sub-committees who shall act, subject to appeal to the whole Council, until their successors are appointed at the following meeting: 1, on Papers and Reports; 2, on Members; 3, on Fellows.

ART. 19. The Nominating Committee shall consist of the Council, and one member or fellow elected by each of the Sections. It shall be the duty of this Committee to meet at the call of the President and elect the general officers for the following meeting of the Association. It shall also be the duty of this Committee to fix the time and place for the next meeting. The Vice President and Secretary of each Section shall be recommended to the Nominating Committee by a sub-committee consisting of the Vice President, Secretary, and three members or fellows elected by the Section.

MEETINGS.

ART. 20. The Association shall hold a public meeting annually, for one week or longer, at such time and place as may be determined by vote of the Nominating Committee, and the preliminary arrangements for each meeting shall be made by the Local Committee, in conjunction with the Permanent Secretary and such other persons as the Council may designate.

ART. 21. A General Session shall be held at 10 o'clock, A. M., on the first day of the meeting, and at such other times as the Council may direct.

SECTIONS AND SUBSECTIONS.

ART. 22. The Association shall be divided into Sections, namely:—A, *Mathematics and Astronomy;* B, *Physics;* C, *Chemistry, including its application to agriculture and the arts;* D, *Mechanical Science and Engineering;* E, *Geology and Geography;* F, *Zoölogy;* G, *Botany;* H, *Anthropology;* I, *Social and Economic Science.* The Council shall have power to consolidate any two or more Sections temporarily, and such consolidated Sections shall be presided over by the senior Vice President and Secretary of the Sections comprising it.

ART. 23. Immediately on the organization of a Section there shall be three fellows elected by ballot after open nomination, who, with the Vice President and Secretary and the Vice President and Secretary of the preceding meeting shall form its Sectional Committee. The Sectional Committees shall have power to fill vacancies in their own numbers. Meetings of the Sections shall not be held at the same time with a General Session.

ART. 24. The Sectional Committee of any Section may at its pleasure form one or more temporary Subsections, and may designate the officers thereof. The Secretary of a Subsection shall, at the close of the meeting, transmit his records to the Secretary of the Section.

ART. 25. A paper shall not be read in any Section or Subsection until it has been received from the Council and placed on the programme of the day by the Sectional Committee.

SECTIONAL COMMITTEES.

ART. 26. The Sectional Committees shall arrange and direct the business of their respective Sections. They shall prepare the daily programmes and give them to the Permanent Secretary for printing at the earliest moment practicable. No titles of papers shall be entered on the daily programmes except such as have passed the Council. No change shall be made in the programme for the day in a Section without the consent of the Sectional Committee. The Sectional Committees may refuse to place the title of any paper on the programme; but every such title, with the abstract of the paper or the paper itself, must be returned to the Council with the reasons why it was refused.

ART. 27. The Sectional Committees shall examine all papers and abstracts referred to the Sections, and they shall not place on the programme

any paper inconsistent with the character of the Association; and to this end they have power to call for any paper, the character of which may not be sufficiently understood from the abstract submitted.

Papers and Communications.

Art. 28. All members and fellows must forward to the Permanent Secretary, as early as possible, and when practicable before the convening of the Association, full titles of all the papers which they propose to present during the meeting, with a statement of the time that each will occupy in delivery, and also such abstracts of their contents as will give a general idea of their nature; and no title shall be referred by the Council to the Sectional Committee until an abstract of the paper or the paper itself has been received.

Art. 29. If the author of any paper be not ready at the time assigned, the title may be dropped to the bottom of the list.

Art. 30. Whenever practicable, the proceedings and discussions at General Sessions, Sections and Subsections shall be reported by professional reporters, but such reports shall not appear in print as the official reports of the Association unless revised by the Secretaries.

Printed Proceedings.

Art. 31. The Permanent Secretary shall have the Proceedings of each meeting printed in an octavo volume as soon after the meeting as possible, beginning one month after adjournment. Authors must prepare their papers or abstracts ready for the press, and these must be in the hands of the Secretaries of the Sections before the final adjournment of the meeting, otherwise only the titles will appear in the printed volume. The Council shall have power to order the printing of any paper by abstract or title only. Whenever practicable, proofs shall be forwarded to authors for revision. If any additions or substantial alterations are made by the author of a paper after its submission to the Secretary, the same shall be distinctly indicated. Illustrations must be provided for by the authors of the papers, or by a special appropriation from the Council. Immediately on publication of the volume, a copy shall be forwarded to every member and fellow of the Association who shall have paid the assessment for the meeting to which it relates, and it shall also be offered for sale by the Permanent Secretary at such price as may be determined by the Council. The Council shall also designate the institutions to which copies shall be distributed.

LOCAL COMMITTEE.

ART. 32. The Local Committee shall consist of persons interested in the objects of the Association and residing at or near the place of the proposed meeting. It is expected that the Local Committee, assisted by the officers of the Association, will make all essential arrangements for the meeting, and issue a circular giving necessary particulars, at least one month before the meeting.

LIBRARY OF THE ASSOCIATION.

ART. 33. All books and pamphlets received by the Association shall be in the charge of the Permanent Secretary, who shall have a list of the same printed and shall furnish a copy to any member or fellow on application. Members and fellows who have paid their assessments in full shall be allowed to call for books and pamphlets, which shall be delivered to them at their expense, on their giving a receipt agreeing to make good any loss or damage and to return the same free of expense to the Secretary at the time specified in the receipt given. All books and pamphlets in circulation must be returned at each meeting. Not more than five books, including volumes, parts of volumes, and pamphlets, shall be held at one time by any member or fellow. Any book may be withheld from circulation by order of the Council. [The Library of the Association was, by vote of the Council in 1895, placed on deposit in the Library of the University of Cincinnati, Ohio. Members can obtain the use of books by writing to the Librarian of the University Library, Cincinnati, Ohio.]

ADMISSION FEE AND ASSESSMENTS.

ART. 34. The admission fee for members shall be five dollars in addition to the annual assessment. On the election of any member as a fellow an additional fee of two dollars shall be paid.

ART. 35. The annual assessment for members and fellows shall be three dollars.

ART. 36. Any member or fellow who shall pay the sum of fifty dollars to the Association, at any one time, shall become a Life Member, and as such, shall be exempt from all further assessments, and shall be entitled to the Proceedings of the Association. All money thus received shall be invested as a permanent fund, the income of which, during the life of the member, shall form a part of the general fund of the Association; but, after his death, shall be used only to assist in original research, unless otherwise directed by unanimous vote of the Council.

ART. 37. All fees and assessments must be paid to the Permanent Secretary, who shall give proper receipts for the same.

ACCOUNTS.

ART. 38. The accounts of the Permanent Secretary and of the Treasurer shall be audited annually, by Auditors appointed by the Council.

ALTERATIONS OF THE CONSTITUTION.

ART. 39. No part of this Constitution shall be amended or annulled, without the concurrence of three-fourths of the members and fellows present in General Session, after notice given at a General Session of a preceding meeting of the Association.

MEMBERS

OF THE

AMERICAN ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE.[1]

PATRONS.[2]

THOMPSON, MRS. ELIZABETH, Stamford, Conn. (22).
LILLY, GEN. WILLIAM, Mauch Chunk, Pa. (28). (Died Dec. 1, 1893.)
HERRMAN, MRS. ESTHER, 59 West 56th St., New York, N. Y. (29).

CORRESPONDING MEMBERS.[3]

Warington, Robert, F.R.S., Rothamsted, Harpenden, England (40). **C**

MEMBERS.[4]

Abbe, Cleveland, jr., 2017 I St., Washington, D. C. (44). **E**
Abraham, Abraham, Brooklyn, N. Y. (43).
Adams, C. E., M.D., Ballentine Gym., New Brunswick, N. J. (43). **F**
Aldis, Owen F., 230 Monadnock Block, Chicago, Ill. (41). **H**
Allderdice, Wm. H., P. A. Engineer, U. S. Navy, care Navy Department, Washington, D. C. (33). **D**

[1] The numbers in parentheses indicate the meeting at which the member was elected. The black letters at the end of line indicate the sections to which members elect to belong. The Constitution requires that the names of all members two or more years in arrears shall be omitted from the list, but their names will be restored on payment of arrearages. Members not in arrears are entitled to the annual volume of Proceedings bound in paper. *The payment of ten dollars at one time entitles a member to the subsequent volumes to which he may be entitled, bound in cloth, or by the payment of twenty dollars, to such volumes bound in half morocco.*

[2] Persons contributing one thousand dollars or more to the Association are classed as Patrons, and are entitled to the privileges of members and to the publications. The names of Patrons are to remain permanently on the list.

[3] See ARTICLE VI of the Constitution.

[4] Any Member or Fellow may become a Life Member by the payment of fifty dollars. The income of the money derived from a Life Membership is used for the general purposes of the Association during the life of the member; afterwards it is to be used to aid in original research. Life Members are exempt from the annual assessment, and are entitled to the annual volume. The names of Life Members are printed in small capitals in the regular list of Members and Fellows.

Allen, Miss Augusta A., 42 Coulter St., Germantown, Pa. (44). **G**
Allen, J. M., Hartford, Conn. (22). **D**
Allen, John Robins, 18 S. Ingalls St., Ann Arbor, Mich. (45). **B D**
Allen, Walter S., 34 S. Sixth St., New Bedford, Mass. (39). **C I**
Anderson, Alexander P., Botanist, Clemson College, S. C. (45). **G**
Anderson, Frank P., Epworth, Iowa (46).
Andrews, Wm. C., care Claflin Thayer & Co., P. O. Box 2000, New York, N. Y. (46).
Appleton, Rev. Edw. W., D.D., Ashbourne, Montgomery Co., Pa. (28).
Archambault, U. E., P. O. Box 1944, Montreal, P. Q., Can. (31).
Arms, Miss Jennie M., 18 W. Cedar St., Boston, Mass. (44). **F**
Atkinson, Jno. B., Earlington, Hopkins Co., Ky. (26). **D**
Atwood, Dr. Charles, Moravia, N. Y. (45). **G**
AVERY, SAMUEL P., 4 E. 38th St., New York, N. Y. (36).
Ayer, Edward Everett, Room 12, The Rookery, Chicago, Ill. (37). **H**
Ayres, Horace B., Allamuchy, N. J. (40).

Backus, Truman J., LL.D., Pres. Packer Institute, Brooklyn, N. Y. (43).
Baker, A. G., Springfield, Mass. (44).
Baker, O. M., 499 Main St., Springfield, Mass. (44).
Balch, Samuel W., Yonkers, N. Y. (43).
Balderston, C. Canby, 35 S. Tenth St., Philadelphia, Pa. (33). **B**
Baldwin, Mrs. G. H., 3 Madison Ave., Detroit, Mich. (34). **H**
Baldwin, Herbert B., 215 Market St., Newark, N. J. (43).
Baldwin, Prof. J. Mark, Princeton Univ., Princeton, N. J. (46). **H**
Bancroft, Alonzo C., Elma, Erie Co., N. Y. (41).
Banes, Charles H., 1107 Market St., Philadelphia, Pa. (31). **D**
BANGS, LEMUEL BOLTON, M.D., 127 E. 34th St., New York, N. Y. (36).
Bannan, John F., North Andover, Mass. (44). **C**
Barber, D. H., P. O. Box 83, Springville, Linn Co., Iowa (37).
Barbour, Prof. Ervin H., Univ. of Nebraska, Lincoln, Neb. (45). **E**
Bardwell, Elisabeth M., Mt. Holyoke College, South Hadley, Mass. (46).
BARGE, B. F., Mauch Chunk, Pa. (33).
Barker, Mrs. Martha M., 26 Eleventh St., Lowell, Mass. (31). **E H**
Barker, Mrs. Mary E., Collinsville, Conn. (45).
Barnett, Miss Katie Porter, Madison, Georgia (44). **A H**
Barnhart, Arthur M., 185 Monroe St., Chicago, Ill. (42).
Barton, G. E., 27 Main St., Millville, N. J. (46). **C**
Barton, Prof. Samuel M., Kernstown, Va. (43). **A**
Bates, George Williams, 32033 Buhl Block, Detroit, Mich. (46). **H I**
Bausch, Henry, P. O Drawer 1033, Rochester, N. Y. (41).
Baxter, James N., care H. E. and C. Baxter, cor Division and Bedford Sts., Brooklyn, N. Y. (36).
Beach, Spencer Ambrose, N. Y. Experiment Station, Geneva, N. Y. (41). **G**
Bean, Thos. E., Box 441, Galena, Ill. (28). **F**
Becher, Franklin A., 406 Irving Place, Milwaukee, Wis. (41). **I A**

Beckwith, Miss Florence, 394 Alexander St., Rochester, N. Y. (45). G
Bell, Miss Clara, Springfield, Mass. (43).
BELL, C. M., M.D., 320 Fifth Ave., New York, N. Y. (36).
Bennett, Henry C., 256 W. 42nd St., New York, N. Y. (48).
Benneville, James S. de, 128 S. 7th St., Philadelphia, Pa. (46). C
Bickford, Miss Elizabeth E., Ph.D., Vassar College, Poughkeepsie, N. Y. (46). F
Biddle, James G., 944 Drexel Building, Philadelphia, Pa. (39).
Bien, Julius, 140 Sixth Ave., New York, N. Y. (34). E H
Bigelow, Willard Dell, Chem. Div., Dept. of Agric., Washington, D. C. (44). C
Biggar, Hamilton F., M.D., 176 Euclid Ave., Cleveland, Ohio (40). B F
Billings, Edgar F., 165 High St., Boston, Mass. (44). C
BISHOP, HEBER R., Mills Building, New York, N. Y. (36).
Bitting, Prof. A. W., Purdue Univ., Lafayette, Ind. (46.)
Blair, Mrs. Helen Quinche, The Ortiz, East Fourth and Sycamore Sts., Cincinnati, Ohio (40). C
Blatchford, Eliphalet W., 375 N. La Salle St., Chicago, Ill. (17). F
Bleile, Albert M., M.D., 342 S. Fourth St., Columbus, Ohio (37). F
BLISH, W. G., Niles, Mich. (33). B D
Bodine, Donaldson, Prof. of Zoölogy and Geology, Wabash College, Crawfordsville, Ind. (45). E F
Bogue, Rev. Horace P. V., Avon, N. Y. (41). H I
Bohannan, Prof. Rosser Daniel, Ohio State University, Columbus, Ohio (46). A
Booraem, J. V. V., 204 Lincoln Place, Brooklyn, N. Y. (36).
Borner, William, The Majestic, 52 and 54 Walton Place, Chicago, Ill. (44).
Boutwell, John Mason, 10 Weld Hall, Cambridge, Mass. (46). E
Bowker, R. R., 28 Elm St., New York, N. Y. (43). B
Boyd, David, A.M., Greeley, Col. (46). I
Boyd, James E., Ohio State University, Columbus, Ohio (46). B
Boynton, May O., Ph.B., 69 North Prospect St., Burlington, Vt. (44). C
BRACKENRIDGE, GEO. W., San Antonio, Texas (41). I
Brackett, Byron B., Ph.D., 722 Union St., Schenectady, N. Y. (46). B
BRADLEY, ARTHUR C., Newport, N. H. (43).
Bradley, Charles S., P. O. Box 259, Avon, N. Y. (40).
BRADLEY, M. J., 36 Hart St., Brooklyn, N. Y. (43).
Bradley, Milton, Springfield, Mass. (44). B
Bramwell, Geo. W., 335 Broadway, New York, N. Y. (43). D
Brayton, Miss Sarah H., M.D., Evanston, Ill. (33).
Brewster, Mrs. Mary S., Mountainville, Orange Co., N. Y. (43).
Brice, Judge Albert G., 19 Camp St., New Orleans, La. (32). H
Briggs, Thomas B., 115 E. 73d St., New York, N. Y. (43).
Brigham, Prof. Albert P., Hamilton, Madison Co., N. Y. (41).
Britton, Wiley, Kansas City, Kansas (40). F
Brooks, Prof. Wm. P., Amherst, Mass. (38). C F
Brown, Samuel B., Morgantown, W. Va. (40). E

Browning, Philip Embury, Kent Chemical Laboratory, Yale University, New Haven, Conn. (46). **C**
Brundage, Albert H., Ph.G., M.D., 1153 Gates Ave., Brooklyn, N. Y. (43). **F G H**
Bryant, Miss D. L., 998 Spring Garden St., Greensboro, N. C. (42). **E**
Buckingham, Chas. L., 195 Broadway, New York, N. Y. (28).
Buffum, Prof. Burt C., State University, Laramie, Wyo. (42). **G**
Bull, Prof. Storm, Madison, Wis. (44). **D**
Burke, Arthur N., A.B., Principal of Monson Academy, Monson, Mass. (44).
Burr, Mrs. Laura E., Commercial Hotel, Lansing, Mich. (34). **B**
Burt, Milo Cudworth, Amherst, Mass. (44). **C**
Burton, Prof. Alfred E., Massachusetts Institute of Technology, Boston, Mass. (40). **E**
Burwell, Arthur W., Ph.D., 208 Superior St., Cleveland, Ohio (37).

Caldwell, Prof. Frank C., Ohio State University, Columbus, Ohio (46).
Calkins, Dr. Marshall, Springfield, Mass. (29).
Cannon, George L., jr., High School, Denver, Col. (39). **F H**
Card, Fred W., Prof. of Horticulture, University of Nebraska, Lincoln, Neb. (45). **G**
Carpenter, Ford A., U. S. Weather Bureau, San Diego, Cal. (44). **B**
CARTER, JAMES C., 277 Lexington Ave., New York, N. Y. (36).
Carter, John E., Knox and Coulter Sts., Germantown, Pa. (33). **B H**
Carter, Prof. Truman Post, 872 Grove St., Jacksonville, Ill. (46). **F G**
Cary, Albert A., 28 Cliff St., New York, N. Y. (36). **D**
Cary, Mrs. Elizabeth M. L., Buffalo, N. Y. (45). **E**
Chadbourn, Erlon R., Lewiston, Me. (29).
Chaney, Prof. Lucian W., Jr., Carleton College. Northfield, Minn. (45).
Chase, R. Stuart, 53 Summer St., Haverhill, Mass. (18). **F**
Child, C. D., Ithaca, N. Y. (44). **B**
Chrystie, Wm. F., Hastings-on-Hudson, New York, N. Y. (36).
Church, Royal Tyler, Turin, Lewis Co., N. Y. (38). **D F**
Clancy, Michael Albert, 1426 Corcoran St., Washington, D. C. (40). **H**
Clapp, Geo. H., 116 Water St., Pittsburg, Pa. (33). **H C**
Clark, Alex. S., Westfield, N. J. (33).
Clark, Joseph E., M.D., 184 Clinton St., Brooklyn, N. Y. (43). **A E**
Clark, Oliver Durfee, 590 Halsey St., Brooklyn, N. Y. (41). **F E**
Clark, S. Wellman, M.D., 110 Mercer St., Jersey City, N. J. (44). **H**
Clark, Thomas H., 22 Lancaster St., Worcester, Mass. (40).
Clark, Wm. Brewster, M.D., 50 E. 31st St., New York, N. Y. (33). **F C**
Clough, Albert L., S.B., Box 14, Manchester, N. H. (45). **B**
Cluett, J. W. Alfred, Troy, N. Y. (43).
Coates, Prof. Charles E., Ph.D., University of Louisiana, Baton Rouge, La. (46). **C**
Cobleigh, Wm. Merriam, E.M., Bozeman, Mont. (45). **C**
COE, HENRY W., M.D., "The Marquam," Portland, Oregon (32). **H F**

Coit, J. Milner, Ph.D., Saint Paul's School, Concord, N. H. (33). **B C E**
Colgate, Abner W., Morristown, N. J. (44).
Colie, Edw. M., East Orange, N. J. (30). **E I**
Collie, Prof. Geo. L., Beloit College, Beloit, Wis. (42). **E**
Collin, Rev. Henry P., Coldwater, Mich. (37). **H**
Colton, Buel P., Normal, McLean Co., Ill. (34). **F**
Comstock, Dr. T. Griswold, 3401 Washington Ave., St. Louis, Mo. (29). **F H**
Conant, Miss E. Ida, 42 W. 48th St., New York, N. Y. (33). **H I F**
Conklin, Prof. Roland E., A.M., Eureka College, Eureka, Ill. (42). **F**
Cook, Dr. Charles D., 183 Pacific St., Brooklyn, N. Y. (25).
Cook, Melville T., Greencastle, Ind. (45). **G**
Coon, Henry C., M.D., Ph.D., Alfred Univ., Alfred, N. Y. (29). **B C F**
Cope, Thos. P., Awbury, Germantown, Pa. (33). **I**
Copeland, Edwin Bingham, Monroe, Wis. (45). **G**
Corbitt, James H., University of Virginia, Charlottesville, Va. (44).
Corcoran, Dr. Luke, Maple St., Springfield, Mass. (44). **H**
Cordeiro, F. J. B., Navy Dep't, Washington, D. C. (46). **A B**
Cornman, Oliver P., 2252 N. 20th St., Philadelphia, Pa. (46). **H**
Courtis, William Munroe, 449 Fourth Ave., Detroit, Mich. (46). **E C G**
Cowell, Jno. F., Buffalo, N. Y. (35).
Cowles, James Lewis, Farmington, Conn. (44).
Cox, Charles F., Pres. Council Scientific Alliance of New York, Grand Central Depot, New York, N. Y. (43).
Crafts, Robert H., 2329 S. 6th St., Minneapolis, Minn. (32). **I B**
Craig, John, Horticulturist, Exper. Farms, Ottawa, Ontario, Can. (41).
Crawford, John, Leon, Nicaragua, C. A. (40). **E H**
Crawley, Edwin S., Ph.D., Springfield Ave., Chestnut Hill, Philadelphia, Pa. (45).
CROWELL, A. F., Woods Holl, Mass. (30). **C**
Cruikshank, James, LL.D., 206 S. Oxford St, Brooklyn, N. Y. (36).
Cunningham, Francis A., 1613 Wallace St., Philadelphia, Pa. (33). **D E B**
Cunningham, Prof. Susan J., Swarthmore College, Swarthmore, Pa. (38). **A**
Cuntz, Johannes H., 325 Hudson St., Hoboken, N. J. (36).
Curtis, William E., Post Building, Washington, D. C. (40). **H I**
Cutler, Dr. Andrew S., Kankakee, Ill. (32). **I E**

Dall, Mrs. Caroline H., Washington, D. C. (46).
DALY, HON. CHARLES P., 84 Clinton Place, New York, N. Y. (36). **E H**
Dana, Charles L., 50 W. 46th St., New York, N. Y. (46). **H**
Davenport, Charles Benedict, Ph.D., 11 Francis Ave., Cambridge, Mass. (46).
Davenport, Prof. Eugene, Dean of the College of Agriculture, University of Illinois, Champaign, Ill. (39).
Davidson, R. J., Experiment Station, Blacksburgh, Va. (40). **C**
Davis, Abial B., A.M., 129 East Lincoln Ave., Mt. Vernon, N. Y. (44). **A**

Davis, Bradley Moore, Dep't of Botany, University of Chicago, Chicago, Ill. (45). **G**
Davis, Prof. Charles A., Alma, Mich. (46). **G**
Davis, J. J., M.D., 1119 College Ave., Racine, Wis. (31). **F G**
Davison, John M., 60 Oxford St., Rochester, N. Y. (38). **C**
Deeds, Edward A., Denison University, Granville, Ohio (45). **B**
DeForest, Henry S. (32). **H I**
Deghuée, Joseph A., 247 Harrison St., Brooklyn, N. Y. (40). **C**
Delafield, Maturin L., jr., Fieldston, Riverdale, New York, N. Y. (43). **G**
Dewey, L. H., Dep't of Agriculture, Washington, D. C. (40). **F**
Dinsmore, Prof. Thos. H., Jr., Emporia, Kan. (29). **B C**
Dittenhoefer, A. J., 96 Broadway, New York, N. Y. (36).
Dixon, Roland B., Peabody Museum, Cambridge, Mass. (46). **H**
Dodge, Chas. Wright, M.S., Univ. of Rochester, Rochester, N. Y. (39). **F**
Dodge, Philip T., Tribune Building, New York, N. Y. (44). **B D**
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Flynn, Oscar R., Columbus, Ohio (46). **C**
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Fretz, Dr. John Edgar, Doylestown, Bucks Co., Pa. (46). **F G H**
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Hopkins, Grant S., Ithaca, N. Y. (41). **F**
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Keeler, Aaron Benedict, Ridgefield, Conn. (45) **I**
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Kellogg, John H., M.D., Battle Creek, Mich. (24). **F**
Kelsey, Rev. F. D., Sc.D., Toledo, Ohio (46).
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Woodhull, John Francis, Teachers' College, Morningside Heights, New York, N. Y. (43).
Woodrow, Miss Marion W., Winthrop Normal College, Rock Hill, S.C. (45). E C
Woodworth, William McMichael, Ph.D., 149 Brattle St., Cambridge, Mass. (44). F
Worcester, Dean C., Ann Arbor, Mich. (46). F H
Wright, Jonathan, M.D., 73 Remsen St., Brooklyn, N. Y. (43).
Wright, John S., care Eli Lilly & Co., Indianapolis, Ind. (42). G
Wright, Rufus, 333–339 Lake St., Chicago, Ill. (37). B
Wunderlich, Frederick W., M.D., 165 Remsen St., Brooklyn, N. Y. (45).

Youmans, Mrs. Celia G., Mount Vernon, N. Y. (36).
Youmans, Vincent J., Mt. Vernon, N. Y. (43).

[847 PATRONS, CORRESPONDING MEMBERS AND MEMBERS.]

NOTE.—The omission of an address in the foregoing list indicates that letters mailed to that last printed were returned as uncalled for. Information of the present address of the members so indicated is requested by the PERMANENT SECRETARY.

SURVIVING FOUNDERS.

[At the Brooklyn Meeting, 1894, a resolution was unanimously adopted by which all surviving founders of the Association who have maintained an interest in science were made Honorary Life Members of the Association in recognition of their pioneer work in American Science.]

BOYÉ, MARTIN H., Coopersburg, Pa.
DIXWELL, EPES S., Cambridge, Mass.
HALL, JAMES, Albany, N. Y.
HUBBARD, OLIVER PAYSON, New York, N. Y.
LINDSLEY, J. BERRIEN, Nashville, Tenn.
WEST, CHARLES E., Brooklyn, N. Y.

HONORARY FELLOWS.[1]

ROGERS, PROF. WILLIAM B., Boston, Mass. (1). 1881. (Born Dec. 7, 1804. Died May 30, 1882.) **B E**

CHEVREUL, MICHEL EUGÈNE, Paris, France (35). 1886. (Born Aug. 31, 1786. Died April 9, 1889.) **C**

GENTH, DR. F. A., 3937 Locust St., Philadelphia, Pa. (24). 1888. (Born May 17, 1820. Died Feb. 2, 1892.) **C E**

HALL, PROF. JAMES, Albany, N. Y. (1). 1890. **E F**

GOULD, DR. BENJAMIN APTHORP, Cambridge, Mass. (2). 1895. (Born Sept. 27, 1824. Died Nov. 26, 1896.) **A B**

LEUCKART, PROF. RUDOLF, Leipsic, Saxony, Germany. (44). 1895. **F**

GIBBS, PROF. WOLCOTT, Newport, R. I. (45) 1896. **C B**

FELLOWS.[2]

Abbe, Professor Cleveland, Meteorologist, Weather Bureau, Department of Agriculture, Washington, D. C. (16). 1874. **B A**

Abbe, Dr. Robert, 11 W. 50th St., New York, N. Y. (36). 1892.

Abert, S. Thayer, 1108 G St., N. W., Washington, D. C. (30). 1891. **A B D E I**

Adriance, John S., 231 Broadway, New York, N.Y. (39). 1895. **C**

Alden, Prof. Geo. I., Worcester, Mass. (33). 1885. **D**

Aldrich, Prof. William Sleeper, West Virginia University, Department Mechanical Engineering, Morgantown, W. Va. (43). 1897. **D**

Alvord, Major Henry E., Lewinsville, Fairfax Co., Va. (29). 1882. **I**

Alwood, Prof. Wm. B., Agricultural and Mechanical College and Experiment Station, Blacksburg, Va. (39). 1891. **F**

Andrews, Prof. Launcelot W., Iowa City, Iowa (39). 1891. **C**

Anthony, Prof. Wm. A., 5 Beekman St., Temple Court, New York, N. Y. (28). 1880. **B**

Arthur, J. C., Lafayette, Ind. (21). 1883. **G**

Ashmead, Wm. H., 1833 M St., N. W., Washington, D. C. (40). 1892. **F**

Atkinson, Edward, 31 Milk St., Boston, Mass. (29). 1881. **I D**

Atkinson, George F., Cornell University, Ithaca, N. Y. (39). 1892. **G**

Atwater, Prof. W. O., Wesleyan University, Middletown, Conn. (29). 1882. **C**

Atwell, Charles B., 1938 Sherman Ave., Evanston, Ill. (36). 1890. **G**

Auchincloss, Wm. S., Bryn Mawr, Pa. (29). 1886. **D A**

Austen, Prof. Peter T., 99 Livingston St., Brooklyn, N. Y. (44). 1896. **C**

Avery, Elroy M., Ph.D., LL.D., 657 Woodland Hills Ave., Cleveland, Ohio (37). 1889. **B**

Ayres, Prof. Brown, Tulane University, New Orleans, La. (31). 1885. **B**

[1] See ARTICLE VI of the Constitution. [2] See ARTICLE IV of the Constitution.

*** The number in parenthesis indicates the meeting at which the member joined the Association; the date following is the year when made a Fellow; the black letters at end of line are those of the sections to which the Fellow belongs.

When the name is given in small capitals, it designates that the Fellow is also a Life Member.

Babcock, Prof. S. Moulton, Madison, Wis. (33). 1885. **C**
Bailey, E. H. S., Lawrence, Douglas Co., Kan. (25). 1889. **C E**
Bailey, Prof. Liberty H., Cornell University, Ithaca, N. Y. (34). 1887. **G**
Baker, Frank, M.D., 1804 Columbia Road, Washington, D. C. (31). 1886. **F H**
Baker, Marcus, U. S. Geological Survey, Washington, D. C. (30). 1882. **A**
Ballard, Harlan H., 50 South St., Pittsfield, Mass. (31). 1891. **E F**
BARKER, PROF. G. F., 3909 Locust St., Philadelphia, Pa. (18). 1875. **B C**
Barnard, Edward E., carė Yerkes Observatory, Lake Geneva, Williams Bay P.O., Wis. (26). 1883. **A**
Barnes, Chas. Reid, Prof. of Botany, University of Wisconsin, 616 Lake St., Madison, Wis. (33). 1885. **G**
Barnum, Miss Charlotte C., Ph.D., 144 Humphrey St., New Haven, Conn. (36). 1896. **A**
Barrows, Walter B., Agricultural College, Ingham Co., Mich. (40). 1897. **F**
Bartlett, Prof. Edwin J., Dartmouth College, Hanover, N. H. (28). 1883. **C**
Bartlett, John R., Commander U. S. N., Lonsdale, R. I. (30). 1882. **E B**
Bartley, Elias H., M.D., 21 Lafayette Ave., Brooklyn, N. Y. (33). 1894. **C**
Barus, Carl, Ph.D., Wilson Hall, Brown University, Providence, R. I. (33). 1887. **B**
Bascom, Miss Florence, Bryn Mawr College, Bryn Mawr, Pa. (42). 1897. **E**
Baskerville, Charles, University of North Carolina, Chapel Hill, N. C. (41). 1894. **C**
Bassett, Homer F., Waterbury, Conn. (23). 1874. **F**
Bauer, Louis A., Ph.D., University of Cincinnati, Cincinnati, Ohio (40). 1892. **A**
Bausch, Edw., P. O. Drawer 1033, Rochester, N. Y. (26). 1883. **A B C F**
Beal, Prof. Wm. James, Agricultural College, Ingham Co., Mich. (17). 1880. **G**
Beardsley, Prof. Arthur, C.E., Ph.D., Swarthmore College, Swarthmore, Del. Co., Pa. (33). 1885. **D**
Beauchamp, Rev. Wm. M., Baldwinsville, N. Y. (34). 1886. **H**
Becker, Dr. Geo. F., U. S. Geol. Survey, Washington, D. C. (36). 1890. **E**
Bedell, Frederick, Ph.D., Cornell University, Ithaca, N. Y. (41). 1894. **B A**
Bell, Alex. Melville, 1525 35th St., Washington, D. C. (31). 1885. **H**
Bell, Robert, M.D., Ass't Director Geological Survey, Ottawa, Ontario, Can. (38). 1889. **E F**
Beman, Wooster W., 19 S. 5th St., Ann Arbor, Mich. (34). 1886. **A**
BENJAMIN, MARCUS, Smithsonian Institution, Washington, D. C. (27). 1887. **C**
Benjamin, Rev. Raphael, M.A., Hotel Premier, 72nd St., New York, N. Y. (34). 1887. **E F G H**
Bessey, Prof. Charles E., Univ. of Nebraska, Lincoln, Neb. (21). 1880. **G**
Bickmore, Prof. Albert S., American Museum of Natural History, 8th Ave. and 77th St., Central Park, New York, N. Y. (17). 1880. **H**

Bigelow, Prof. Frank H., U. S. Weather Bureau, Washington, D. C. (36). 1888. **A**

BIXBY, Major W. H., U. S. A., Custom House, Cincinnati, Ohio (34). 1892. **D**

Blackham, George E., M.D., Dunkirk, N. Y. (25). 1883. **F**

Blair, Andrew A., 406 Locust St., Philadelphia, Pa. (44). 1896. **C**

Blake, Clarence J., M.D., 226 Marlborough St., Boston, Mass. (24). 1877. **B F**

Blake, Francis, Auburndale, Mass. (23). 1874. **B A**

Blue, Archibald, Director of the Bureau of Mines, Toronto, Ontario, Can. (35). 1890. **I**

Boas, Dr. Franz, American Museum Natural History, Central Park, New York, N. Y. (36). 1888. **H I**

Boerner, Chas. G., Vevay, Switzerland Co., Ind. (29). 1886. **A B E**

Bolley, Henry L., North Dakota Experiment Station, Fargo, North Dakota. (39). 1892. **G**

BOLTON, DR. H. CARRINGTON, Cosmos Club, Washington, D. C. (17). 1875. **C**

Bond, Geo. M., care of The Pratt & Whitney Co., Hartford, Conn. (33). 1885. **D**

Booth, Miss Mary A., 32 Byers St., Springfield, Mass. 1894. **F I G**

Bowditch, Charles P., 28 State St., Boston, Mass. (43). 1897. **H**

Bowditch, Prof. H. P., Jamaica Plain, Mass. (28). 1880. **F B H**

Bowser, Prof. E. A., Rutgers College, New Brunswick, N. J. (28). 1881.

BOYÉ, MARTIN H., M.D., Coopersburg, Lehigh Co., Pa. (1). 1896. **C**

Boynton, Prof. C. Smith, 69 N. Prospect St., Burlington, Vt. (44). 1896. **C**

Brackett, Prof. C. F., Princeton University, Princeton, N. J. (19). 1875. **B**

Brackett, Richard N., Associate Professor of Chemistry, Clemson College, S. C. (37). **C E**

Bradford, Royal B., Commander U. S. N., care Navy Department, Washington, D. C. (31). 1891. **B D**

Branner, Prof John C., Stanford University, Cal. (34). 1886. **E F**

Brashear, Jno. A., Allegheny, Pa. (33). 1885. **A B D**

Brewer, Prof. Wm. H., New Haven, Conn. (20). 1875. **E F I**

Brinton, D. G., M.D., Media, Pa. (33). 1885. **H**

Bristol, Wm. H., Stevens Institute, Hoboken, N. J. (36). 1894. **A B D**

Britton, N. L., Ph.D., Director-in-chief N. Y. Botanical Garden, 41 E. 49th St., New York, N. Y. (29). 1882. **G E**

Bromwell, Wm., Pratt Institute, Brooklyn, N. Y. (40). 1896.

Brooks, Wm. R., Box 714, Geneva, N. Y. (35). 1886. **A B D G**

Brown, Robert, care of Yale University Observatory, New Haven, Conn. (11). 1874.

Brown, Mrs. Robert, New Haven, Conn. (17). 1874.

Brühl, Gustav, cor. John and Hopkins Sts., Cincinnati, Ohio (28). 1886. **H**

Brush, Charles F., Brush Electric Light Co., Cleveland, Ohio (35). 1886. **B**

BRUSH, PROF. GEORGE J., Yale Univ., New Haven, Conn. (4). 1874. **C E**

Buckhout, W. A., State College, Centre Co., Pa. (20). 1881. F

Burgess, Dr. Thomas J. W., Med. Sup't, Protestant Hospital for the Insane, Montreal, P. Q., Can. (38). 1889. G

Burr, Prof. William H., School of Mines, 41 E. 49th St., New York, N. Y. (31). 1883.

Butler, A. W., Secretary Board of State Charities, Indianapolis, Ind. (30). 1885. F H

Caldwell, Prof. Geo. C., Cornell University, Ithaca, N. Y. (23). 1875. C

Calvin, Prof. Samuel, State University of Iowa, Iowa City, Iowa (37). 1889. E F

Campbell, Prof. Douglas H., Menlo Park, Cal. (34). 1888. G

Campbell, Prof. Edw. D., Ann Arbor, Mich. (44). 1896. C

Canby, William M., 1101 Delaware Avenue, Wilmington, Del. (17). 1878. G

Carhart, Prof. Henry S., University of Michigan. Ann Arbor, Mich. (29). 1881. B

Carleton, M. A., Department Agriculture, Division of Vegetable Pathology, Washington, D. C. (42). 1894. G

Carpenter, Louis G., Agricultural College, Fort Collins, Col. (32). 1889. A B

Carpenter, Capt. W. L., U. S. A., care Adjutant General, Washington, D. C. (24). 1877. F E

Carter, James Madison G., M.D., Waukegan, Ill. (39). 1895. F

Carus, Paul, Ph.D., La Salle, Ill. (40). 1895. H

Casey, Thomas L., 1419 K St., N. W., Washington, D. C. (38). 1892. F

Catlin, Charles A., 133 Hope St., Providence, R. I. (33). 1895. C

Cattell, Prof. James McKeen, Columbia University, New York, N. Y. (44). 1896. B F H I

Chalmot, G. de, Spray, N. C. (44). 1896. C

Chamberlain, Alexander F., Clark University, Worcester, Mass. (38). 1890. H

Chamberlin, T. C., 5041 Madison Ave., Chicago, Ill. (21). 1877. E B F H

Chandler, Prof. C. F., School of Mines, Columbia University, New York, N. Y. (19). 1875. C

Chandler, Prof. Charles Henry, Ripon, Wis. (28). 1883. A

Chandler, Seth C., 16 Craigie St., Cambridge, Mass. (29). 1882. A

Chanute, O., 413 E. Huron St., Chicago, Ill. (17). 1877. D I

Charbonnier, Prof. L. H., University of Georgia, Athens, Ga. (26). 1894. A B D

Chase, Frederick L., Yale University Observatory, New Haven, Conn. (43). 1896.

Cheney, Lellen Sterling, 1081 W. Johnson St., Madison, Wis. (42). 1894. G

Chester, Prof. Albert H., Rutgers College, New Brunswick, N. J. (29). 1882. C F

Chester, Commander Colby M , U. S. N., care Navy Department, Washington, D. C. (28). 1897. E

Christie, James, Pencoyd, Pa. (33). 1894. **D**
Chute, Horatio N., Ann Arbor, Mich. (34). 1889. **B C A**
Clark, Prof. John E., 445 Orange St., New Haven, Conn. (17). 1875. **A**
Clarke, John Mason, Ass't State Geologist and Palæontologist of N. Y., State Hall, Albany, N. Y. (45). 1897. **E**
Clark, Wm. Bullock, Ph.D., Johns Hopkins University, Baltimore, Md. (37). 1891. **E**
Clarke, Prof. F. W., U. S. Geological Survey, Washington, D. C. (18). 1874. **C**
Clarke, Robert, Cincinnati, Ohio (30). 1895. **H**
Claypole, Prof. Edward W., 603 Buchtel Ave., Akron, Ohio (30). 1882. **E F**
Cloud, John W., 974 Rookery, Chicago, Ill. (28). 1886. **A B D**
Cochran, C. B., Food Inspector to State Board of Agriculture, 514 South High St., West Chester, Chester Co., Pa. (43). 1896. **C**
Coffin, Selden J., Prof. of Astronomy, Lafayette College, Easton, Pa. (22). 1874. **A I**
Cogswell, W. B., Syracuse, N. Y. (33). 1891. **D**
COLBURN, RICHARD T., Elizabeth, N. J. (31). 1894. **I F H**
Cole, Prof. Alfred D., Denison University, Granville, Ohio (39). 1891. **B C**
Collin, Prof. Alonzo, Cornell College, Mount Vernon, Iowa (21). 1891. **B C**
Collingwood, Francis, Elizabeth, N. J. (36). 1888. **D**
Colvin, Verplanck, Supt. N. Y. State Adirondack Survey, Albany, N. Y. (28). 1880. **E**
Conant, Prof. L. L., Polytechnic Institute, Worcester, Mass. (39). 1892. **A**
Cook, Prof. Orator F., Huntington, N. Y. (40). 1892. **G**
Cooley, Prof. Le Roy C., Vassar College, Poughkeepsie, N. Y. (19). 1880. **B C**
Cooley, Prof. Mortimer E., University of Michigan, Ann Arbor, Mich. (33). 1885. **D**
Corthell, Elmer L., 27 Pine St., New York, N. Y. (34). 1886. **D I E**
Coulter, Prof. John M., University of Chicago, Chicago, Ill. (32). 1884. **G**
Coville, Frederick V., Department of Agriculture, Washington, D. C. (35). 1890. **G**
Cowles, Alfred H., 656 Prospect St., Cleveland, Ohio (37). 1897. **B C**
Cragin, Francis W., Colorado College, Colorado Springs, Col. (29). 1890. **F E H**
Crampton, Chas. A., M.D., Office of Internal Revenue, Treasury Department, Washington, D. C. (36). 1887. **C**
Crandall, Prof. Charles S., Fort Collins, Col. (40). 1894.
Crawford, Prof. Morris B., Middletown, Conn. (30). 1889. **B**
Crockett, Charles W., Rensselaer Polytechnic Institute, Troy, N. Y. (39). 1894. **A D**
Cross, Prof. Charles R., Massachusetts Institute Technology, Back Bay, Boston, Mass. (29). 1880. **B**
Culin, Stewart, University of Pennsylvania, Philadelphia, Pa. (33). 1890. **H**
Cushing, Frank H., Bureau of Ethnology, Washington, D. C. (40). 1893. **H**

Cushing, Henry Platt, Adelbert College, Cleveland, Ohio (33). 1888. **E**

Dall, William H., Smithsonian Institution, Washington, D. C. (18). 1874. **H F**

Dana, Edward Salisbury, New Haven, Conn. (23). 1875. **B E**

Dana, Gen. James J., U. S. A., 1412 21st St., N. W., Washington, D. C. (40). 1896.

Darton, Nelson H., U. S. Geological Survey, Washington, D. C. (37). 1893.

Davis, C. H., Commander U. S. N., Navy Department, Washington, D. C. (40). 1896.

Davis, Prof. Wm. Morris, Cambridge, Mass. (33). 1885. **E B**

Dawson, Geo. M., S.S.C., F.G.S., Geol. Survey, Ottawa, Ontario, Can. (38). 1895. **E**

Dawson, Sir William, Principal McGill College, Montreal, Can. (10). 1875. **E**

Day, David F., Buffalo, N. Y. (35). 1887. **G**

Day, Fisk H., M.D., 309 Sycamore St., Lansing, Mich. (20). 1874. **E H F**

Dennis, Louis Monroe, Cornell University, Ithaca, N. Y. (43). 1895. **C**

Denton, Prof. James E., Stevens Institute, Hoboken, N. J. (36). 1888. **D B A**

Derby, Orville A., San Paulo, Brazil, S. A. (39). 1890.

Dewey, Frederic P., 702 9th St. N. W., Washington, D. C. (30). 1886. **C E**

Dexter, Julius, Cincinnati, Ohio (30).

Dimmock, George, 679 State St., Springfield, Mass. (22). 1874. **F**

DIXWELL, EPES S., Cambridge, Mass. (1). 1896. **H F**

Dodge, Charles R., U. S. Dep't Agriculture, Washington, D. C. (22). 1874.

Dolbear, Prof. A. Emerson, Tufts College, Mass. (20). 1880. **B**

Doolittle, Prof. C. L., University of Pennsylvania, Philadelphia, Pa. (25). 1885. **A**

Douglass, Andrew E., American Museum of Natural History, Central Park, New York, N. Y. (31). 1885. **H**

DRAPER, DAN'L, Ph.D., Director N. Y. Meteorological Observatory, Central Park, 64th St., Fifth Ave., New York, N. Y. (29). 1881. **B D F A**

Drown, Prof. Thos. M., Lehigh Univ., South Bethlehem, Pa. (29). 1881. **C**

DU BOIS, PROF. AUG. J., New Haven, Conn. (30). 1882. **A B D**

Du Bois, Patterson, Ass't Editor S.S.T., 1031 Walnut St., Philadelphia, Pa. (33). 1887. **H C I**

Dudley, Charles B., Drawer 334, Altoona, Pa. (23). 1882. **C B D**

DUDLEY, WM. L., Prof. of Chemistry, Vanderbilt University, Nashville, Tenn. (28). 1881. **C**

Dudley, Prof. Wm. R., Stanford University, Cal. (29). 1888. **G**

Dunham, Edw. K., 338 E. 26th St., New York, N. Y. (30). 1890.

Dunnington, Prof. F. P., University Station, Charlottesville, Va. (26). 1880. **C**

Du Pont, Francis G., Wilmington, Del. (33). 1896. **A B D**

Durand, Prof. W. F., Ph.D., Cornell University, Ithaca, N.Y. (37). 1890. **B**

Dwight, Prof. William B., Vassar College, Poughkeepsie, N. Y. (30). 1882. **E F**

Eastman, Charles Rochester, Museum Comparative Zoölogy, Cambridge, Mass. (41). 1896. **E**
Eastman, Prof. J. R., U. S. Naval Observatory, Washington, D. C. (26). 1879. **A**
Eccles, Robert G., M.D., 191 Dean St., Brooklyn, N. Y. (31). 1894. **F C**
Eddy, Prof. H. T., Engineering and Mechanics, University of Minnesota, Minneapolis, Minn. (24). 1875. **A B D**
Edison, Thos. A., Orange, N. J. (27). 1878. **B**
Egleston, Prof. Thomas, 35 W. Washington Square, New York, N. Y. (27). 1879. **C D E**
Eichelberger, William Snyder, Ph.D., Nautical Almanac Office, U. S. Naval Observatory, Washington, D. C. (41). 1896. **A**
Eimbeck, William, U. S. Coast and Geodetic Survey, Washington, D. C. (17). 1874. **A B D**
Elkin, William L., Yale University Observatory, New Haven, Conn. (33). 1885. **A**
Ely, Theo. N., Chief of Motive Power, Pennsylvania R. R., Broad St. Station, Philadelphia, Pa. (29). 1886.
Emerson, Prof. Benjamin K., Box 203, Amherst, Mass. (19). 1877. **E F**
Emery, Albert H., Stamford, Conn. (29). 1884. **D B**
Emery, Charles E., Bennett Building, New York, N. Y. (34). 1886. **D B A**
Emmons, S. F., U. S. Geological Survey, Washington, D. C. (26). 1879. **E**
Engelmann, George J., M.D., 336 Beacon St., Boston, Mass. (25). 1875. **F H**
Ewell, Ervin E., 3644 18th St., N. W., Washington, D. C. (40). 1896. **C**
Eyerman, John, "Oakhurst," Easton, Pa. (33). 1889. **E C**

Fairbanks, Henry, Ph.D., St. Johnsbury, Vt. (14). 1874. **B D A**
Fairchild, Prof. H. L., University of Rochester, Rochester, N. Y. (28). 1883. **E F**
Fanning, John T., Consulting Engineer, Kasota Block, Minneapolis, Minn. (29). 1885. **D**
Farlow, Dr. W. G., 24 Quincy St., Cambridge, Mass. (20). 1875. **G**
Farquhar, Henry, Dep't of Agric., Washington, D. C. (33). 1886. **A I G B**
Fernow, Bernhard E., Chief of Forestry Division, Department of Agriculture, Washington, D. C. (31). 1887. **G I**
Ferry, Ervin S., University of Wisconsin, Madison, Wis. (41). 1896.
Firmstone, F., Easton, Pa. (33). 1887. **D**
Fiske, Thos. S., A.M., Ph.D., Columbia University, New York, N. Y. (38). 1891. **A**
Flather, Prof. John J., 160 South St., Lafayette, Ind. (44). 1896. **D**
Fletcher, Miss Alice C., care Peabody Museum, Cambridge, Mass. (29). 1883. **H**
Fletcher, James, Dominion Entomologist, Experimental Farm, Ottawa, Ontario, Can. (31). 1883. **F**

Fletcher, Dr. Robert, Army Medical Museum, Washington, D. C. (29). 1881. **F H**

Flint, Albert S., Washburn Observatory, Madison, Wis. (30). 1887. **A**

Flint, James M., Surgeon U. S. N., Smithsonian Institution, Washington, D. C. (28). 1882. **F**

Ford, Prof. D. R., Elmira, N. Y. (41). 1894. **A B**

Fox, Oscar C., U. S. Patent Office, Washington, D. C. (36). 1891. **B D A**

Franklin, William S., Ames, Iowa (36). 1892.

FRAZER, DR. PERSIFOR, Drexel Building, Room 1042, Philadelphia, Pa. (24). 1879. **E C**

Frazier, Prof. B. W., The Lehigh University, So. Bethlehem, Pa. (24). 1882. **E C**

Frear, Wm., State College, Centre Co., Pa. (33). 1886. **C**

Freer, Prof. Paul C., Ann Arbor, Mich. (39). 1891. **C**

French, Prof. Thomas, jr., Ridgeway Ave., Avondale, Cincinnati, Ohio (30). 1883. **B**

Frisby, Prof. Edgar, U. S. N. Observatory, Washington, D. C. (28). 1880. **A**

Frost, Edwin Brant, Hanover, N. H. (38). 1890. **A B**

Frost, Howard V., Ph.D., Arlington, Mass. (38). 1895. **C**

Fuller, Andrew S., Ridgewood, Bergen Co., N. J. (24). 1882. **F**

Fuller, Prof. Homer T., President Drury College, Springfield, Mo. (35). 1891. **C E**

Fulton, Robert B., Chancellor University of Mississippi, Prof. of Physics and Astronomy, University, Miss. (21). 1887. **B A**

Gaffield, Thomas, 54 Allen St., Boston, Mass. (29). 1889. **C B**

Gage, Prof. Simon Henry, Ithaca, N. Y. (28). 1881. **F**

Galbraith, Prof. John, Toronto, Ontario, Can. (38). 1889.

Galloway, B. T., Dep't of Agriculture, Washington, D. C. (37). 1890. **G**

Gibbs, Prof. J. Willard, New Haven, Conn. (33). 1885. **B**

Gilbert, G. K., U. S. Geological Survey, Washington, D. C. (18). 1874. **E**

Gill, Adam Capen, Cornell University, Ithaca, N. Y. (38). 1894. **E**

Gill, Augustus Herman, Massachusetts Institute Technology, Back Bay, Boston, Mass. (44). 1896. **C**

Gill, Prof. Theo., Columbian University, Washington, D. C. (17). 1874. **F**

Gillman, Henry, 107 Fort St. West, Detroit, Mich. (24). 1875. **H G**

Gilman, Daniel C., President Johns Hopkins University, Baltimore, Md. (10). 1875. **E H**

Glenn, William, 1348 Block St., Baltimore, Md. (33). 1893. **C**

Goessman, Prof. C. A., Massachusetts Agricultural College, Amherst, Mass. (18). 1875. **C**

Goff, E. S., 1113 University Ave., Madison, Wis. (35). 1889.

Gold, Theodore S., West Cornwall, Conn. (4). 1887. **B C**

Golden, Miss Katherine E., Lafayette, Ind. (42). 1897. **G**

Goldschmidt, S. A., Ph.D., 43 Sedgwick St., Brooklyn, N. Y. (24). 1880. **C E B**

Goldsmith, Edw., 658 N. 10th St., Philadelphia, Pa. (29). 1892. **C B**
Gooch, Frank A., Yale University, New Haven, Conn. (25). 1880. **C**
Goodale, Prof. G. L., Botanic Gardens, Cambridge, Mass. (18). 1875. **G**
Goss, Prof. Wm. F. M., Lafayette, Ind. (39). 1896.
GRANT, MRS. MARY J., 36 Division St., Danbury, Conn. (23). 1874. **A**
Gratacap, L. P., Ph.B., 77th St. and 8th Ave., New York, N. Y. (27). 1884. **C E F**
Gray, Prof. Thomas, Terre Haute, Ind. (38). 1889.
Green, Arthur L., Purdue University, Lafayette, Ind. (33). 1888. **C**
Grimes, J. Stanley, 1421 Wesley Ave., Evanston, Ill. (17). 1874. **E H**
Grinnell, George Bird, 346 Broadway, New York, N. Y. (25). 1885. **F E**
Griswold, Leon Stacy, 238 Boston St., Dorchester, Mass. (38). 1893. **E**
Gunckel, Lewis W., 435 W. Second St., Dayton, Ohio (41). 1897. **H**
Guthe, Karl E., Ph.D., Instructor in Physics at the University of Michigan, 904 S. State St., Ann Arbor, Mich. (45). 1897. **B D**

Hague, Arnold, U. S. Geological Survey, Washington, D. C. (26). 1879. **E**
Haines, Reuben, Haines and Chew St., Germantown, Philadelphia, Pa. (27). 1889. **C B**
Hale, Albert C., Ph.D., No. 551 Putnam Ave., Brooklyn, N. Y. (29). 1886. **C B**
Hale, Geo. E., Yerkes Observatory, Williams Bay, Wis. (37). 1891. **A B C**
Hale, William H., Ph.D., 40 First Place, Brooklyn, N. Y. (19). 1874. **I F H C B E A**
Haliburton, R. G., Q C., 99 State St., Boston, Mass. (43). 1895. **H**
Hall, Arthur G., 36 Oakland Ave., Ann Arbor, Mich. (41). 1896. **A B**
Hall, Prof. Asaph, 12 Kirkland Place, Cambridge, Mass. (25). 1877. **A**
Hall, Asaph, jr., University of Michigan, Ann Arbor, Mich. (38). 1890. **A**
Hall, Prof. C. W., Dean College Engineering Met. and Mechan. Arts, University of Minnesota, Minneapolis, Minn. (28). 1883. **D E**
Hall, Prof. Edwin H., 5 Avon St., Cambridge, Mass. (29). 1881. **B**
Hall, Prof. Lyman B., Haverford College, Haverford, Pa. (31). 1884. **C**
Hallock, Albert P., Ph.D., 440 First Ave., New York, N. Y. (31). 1896. **C**
Hallock, Dr. Wm., Columbia University, New York, N. Y. (40). 1893. **B E**
Hallowell, Miss Susan M., Wellesley College, Wellesley, Mass. (33). 1890. **G**
Halsted, Byron D., New Jersey Agricultural Experiment Station, New Brunswick, N. J. (29). 1888. **G**
Halsted, Prof. George Bruce, Austin, Texas (43). 1896.
Hambach, Dr. G., 1319 Lami St., St. Louis, Mo. (26). 1891. **F E**
HANAMAN, C. E., Troy, N. Y. (19). 1883. **F**
Hardy, Prof. A. S., Dartmouth College, Hanover, N. H. (28). 1883. **A**
Hargitt, Prof. Charles W., Syracuse University, Syracuse, N. Y. (38). 1891. **F**
HARKNESS, PROF. WILLIAM, U. S. N. Observatory, Washington, D. C. (26). 1878. **A B C D**
Harris, Abram Winegardner, Sc.D., President Maine State College, Orono, Me. (40). 1895. **C**

Harris, Prof. E. P., Amherst College, Amherst, Mass. (44). 1896.
Harris, Gilbert D., Cornell University, Ithaca, N. Y. (37). 1893. **F E**
Harris, Uriah R., Lieutenant Com'dr, U. S. N., Navy Yard, Boston, Mass. (34). 1886. **A**
Harris, W. T., 1303 P St., N. W., Washington, D. C. (27). 1887. **H I**
Hart, Edw., Ph.D., Lafayette College, Easton, Pa. (33). 1885. **C**
Haskell, Eugene E., U. S. Engineer Office, Sault Ste. Marie, Mich. (39). 1896. **A B D**
HASTINGS, C. S., Sheffield Scientific School of Yale University, New Haven, Conn. (25). 1878. **B**
Haynes, Prof. Henry W., 239 Beacon St., Boston, Mass. (28). 1884. **H**
Hedrick, Henry B., A.B., Glencarlyn, Alexandria Co., Va. (40). 1896.
Hering, Rudolph, Civil and Sanitary Engineer, 100 William St., New York, N. Y. (33). 1885. **D E I**
Herty, Chas. Holmes, Ph.D., University of Georgia, Athens, Ga. (42). 1895. **C**
Hervey, Rev. A. B., Bath, Me. (22). 1879. **F**
Hilgard, Prof. E. W., University of California, Berkeley, Cal. (11). 1874. **C E B**
Hill, David J., President University of Rochester, Rochester, N.Y. (41). 1895. **H I**
Hill, John Edward, Brown University, Providence, R. I. (44). 1897. **D**
Hill, Robert Thomas, U. S. Geological Survey, Washington, D. C. (36). 1889. **E**
Hillyer, Homer W., Ph.D., Chemical Laboratory, University of Wisconsin, Madison, Wis. (42). 1896. **C**
Himes, Prof. Charles F., Carlisle, Pa. (29). 1882. **B C**
Hinrichs, Dr. Gustavus, 3132 Lafayette Ave., St. Louis, Mo. (17). 1874. **C B**
Hitchcock, Albert Spear, Manhattan, Kan. (39). 1892. **G**
HITCHCOCK, PROF. CHARLES H., Hanover, N. H. (11). 1874. **E**
Hobbs, William Herbert, Ph.D., Madison, Wis. (41). 1893. **E**
Hodgkins, Prof. H. L., Columbian University, Washington, D. C. (40). 1896. **A B**
Hoffmann, Dr. Fred., "Rundschau," P. O. Box 1680, New York, N. Y. (28). 1881. **C F**
Holland, W. J., D.D., LL.D., Chancellor Western University of Pennsylvania, Pittsburgh, Pa. (37). 1896. **F**
Hollick, Arthur, Columbia University, New York, N. Y. (31). 1892. **G E**
Holmes, Wm. H., National Museum, Washington, D.C. (30). 1883. **H**
Horsford, Miss Cornelia, 27 Craigie St., Cambridge, Mass. (46) 1897. **H**
Hosea, Lewis M., Johnston Building, Cincinnati, Ohio (30). 1883. **B H**
Hotchkiss, Major Jed., Staunton, Va. (31). 1883. **E H I**
Hough, Prof. G. W., Northwestern University, Evanston, Ill. (15). 1874. **A**
Hovey, Edmund O., American Museum Natural History, Central Park, New York, N. Y. (36). 1895. **C E**
Hovey, Rev. Horace C., 60 High St., Newburyport, Mass. (29). 1883. **E H**

Howard, Prof. Curtis C., 97 Jefferson Ave., Columbus, Ohio (38). 1892. **C**
Howard, Leland O., Dep't of Agriculture, Washington, D. C. (37). 1889. **F**
Howe, Charles S., Prof. of Mathematics, Case School of Applied Science, Cleveland, Ohio (34). 1891. **A**
Howe, Prof. Jas. Lewis, Washington and Lee University, Lexington, Va. (36). 1888. **C**
Howell, Edwin E., 612 17th St., N. W., Washington, D. C. (25). 1891. **E**
Hrdlicka, Alĕs, M.D., 823 Park Ave., New York, N. Y. (46). 1897. **H**
Hubbard, Henry Guernsey, 230 New Jersey Ave., Washington, D. C. (41). 1895. **F**
HUBBARD, PROF. OLIVER PAYSON, 117 W. 55th St., New York, N. Y. (1). 1896.
Hulst, Rev. Geo. D., 15 Himrod St., Brooklyn, N. Y. (29). 1887. **F**
Hunt. Alfred E., 116 Water St., Pittsburgh, Pa. (35). 1891. **C D**
Hunter, Andrew Frederick, Barrie, Ontario, Can. (38). **B H I**
Hyatt, Prof. Alpheus, Natural History Society, Back Bay, Boston, Mass. (18). 1875. **E F**
Hyde, Prof. E. W., Station D, Cincinnati, Ohio (25). 1881. **A**

Iddings, Joseph P., University of Chicago, Chicago, Ill. (31). 1884. **E**

Jack, John G., Jamaica Plain, Mass. (31). 1890. **G**
Jackson, Prof. Charles L., Harvard Univ., Cambridge, Mass. (44). 1895. **C**
Jackson, Robert T., 33 Gloucester St., Boston, Mass. (37). 1890. **F**
Jacobus, David S., Stevens Institute, Hoboken, N. J. (36). 1889. **D B A**
Jacoby, Harold, Columbia University, New York, N. Y. (38). 1891. **A**
Jacoby, Henry S., Associate Prof. of Bridge Engineering and Graphics, Cornell University, Ithaca, N. Y. (36). 1892. **D**
Jastrow, Dr. Jos., University of Wisconsin, Madison, Wis. (35). 1887. **H F**
Jayne, Horace F., 1826 Chestnut St., Philadelphia, Pa. (29). 1884. **F H**
Jeffries, B. Joy, M.D., 15 Chestnut St., Boston, Mass. (29). 1881. **F H**
Jenkins, Edw. H., Drawer 101, New Haven, Conn. (33). 1885. **C**
Jenks, Elisha T., Middleborough, Mass. (22). 1874. **D**
Jesup, Prof. Henry G., Dartmouth College, Hanover, N. H. (36). 1891. **F**
Jesup, Morris K., 44 Pine St., New York, N. Y. (29). 1891. **I**
Jewell, Theo. F., Commander U. S. Navy, Room No. 22, P. O. Building, Buffalo, N. Y. (25). 1882. **B**
Jillson, Dr. B. C., 6045 Bond St., Pittsburgh, Pa. (14). 1881. **E H F**
Johnson, John B., Washington University, St. Louis, Mo. (33). 1886. **D**
Johnson, Otis C., 52 Thayer St., Ann Arbor, Mich. (34). 1886. **C**
Jones, Lewis R., Burlington, Vt. (41). 1894. **G**
Jones, Prof. Marcus E., Salt Lake City, Utah (40). 1893.
Jordan, Prof. David S., Palo Alto, Menlo Park P. O., Cal. (31). 1883. **F**
Julien, A. A., New York Academy of Sciences, New York, N. Y. (24). 1875. **E C**

Kedzie, Mrs. Nellie S., Manhattan, Kan. (34). 1890. **I F**
Kedzie, Prof. Robert C., Agricultural College, Mich. (29). 1881. **C**
Keep, Wm. J., Detroit, Mich. (37). 1897.

Kellerman, Prof. William A., Ohio State University, Columbus, Ohio (41). 1893. G

Kellicott, David S., Ohio State University, Columbus, Ohio (31). 1883. F

Kemp, James F., School of Mines, Columbia University, New York, N. Y. (36). 1888. E

Kendall, Prof. E. Otis, 3826 Locust St., Philadelphia, Pa. (29). 1882. A

Kendrick, Prof. Arthur, Rose Polytechnic Institute, Terre Haute, Ind. (45). 1897. B

Kent, William, Passaic, N. J. (26). 1881. D I

Kenyon, Frederick C., Dep't of Agric., Washington, D. C. (46). 1897. F

Kershner, Prof. Jefferson E., Lancaster City, Pa. (29). 1888. A B

Kinealy, John H., Washington University, St. Louis, Mo. (36). 1891. D

King, F. H., Experiment Station, Madison, Wis. (32). 1892. E F

Kinnicutt, Dr. Leonard P., Polytechnic Institute, Worcester, Mass. (28). 1883. C

Klotz. Otto Julius, 437 Albert St., Ottawa, Ontario, Can. (38). 1889.

Knowlton, Frank H., U. S. National Museum, Washington, D. C. (33). 1893. G E

Kober, Geo. Martin, M.D., 1819 Q St. N. W., Washington, D. C. (40). 1896. H

Kunz, G. F., care Messrs. Tiffany & Co., Union Square, New York, N. Y. (29). 1883. E H C

Lacoe, Ralph D., Pittston, Pa. (31). 1898. E F

Ladd, Prof. E. F., Agricultural College, Fargo, No. Dakota (36). 1889. C

Laflamme, Prof. J. C. K., Laval University, Quebec, Can. (29). 1887. E B

LaFlesche, Francis, Indian Bureau, Interior Dep't, Washington, D. C. (33). 1885. H

Lamb, Daniel S., M.D., 800 10th St., N. W., Washington, D. C. (40). 1894. H

Lambert, Preston A., 215 S. Center St., Bethlehem, Pa. (41). 1896. A

Landreth, Olin H., Prof. of Civil Engineering, Union College, Schenectady, N. Y. (28). 1883. D

Langenbeck, Karl, 27 Orchard St., Zanesville, Ohio (39). 1896. C

Langley, Prof. J. W., Case School of Applied Science, Cleveland, Ohio (23). 1875. C B

Langley, Prof. S. P., Secretary Smithsonian Institution, Washington, D. C. (18). 1874. A B

Lanza, Prof. Gaetano, Massachusetts Institute of Technology, Back Bay, Boston, Mass. (29). 1882. D A B

Lattimore, Prof. S. A., University of Rochester, Rochester, N. Y. (15). 1874. C

Laudy, Louis H., Ph.D., School of Mines, Columbia University, New York, N. Y. (28). 1890. C

Lazenby, Prof. Wm. R., Columbus, Ohio (30). 1882. B I

Leach, Miss Mary F., Mt. Holyoke College, South Hadley, Mass. (44). 1896. C

LeBrun, Mrs. Michel, 8 Mountain Ave., Montclair, New Jersey. (35). 1892. **F**

LeConte, Prof. Joseph, University of California, Berkeley, Cal. (29). 1881. **E F**

Ledoux, Albert R., Ph.D., 9 Cliff St., New York, N. Y. (26). 1881. **C**

Leeds, Prof. Albert R., Stevens Institute, Hoboken, N. J. (23). 1874. **C F**

Lefavour, Prof. Henry, Williams College, Williamstown, Mass. (42). 1894.

Lehmann, G. W., Ph.D., 412 E. Lombard St., Baltimore, Md. (30). 1885. **C B**

Lennon, William H., Brockport, N. Y. (31). 1894. **G C**

Lesley, Prof. J. Peter, P. O. Box 93, Milton, Mass. (2). 1874. **E**

Leverett, Frank, Denmark, Iowa (37). 1891. **E**

Libbey, Prof. William, jr., Princeton, N. J. (29). 1887. **E F**

Lindenthal, Gustav, C.E., 45 Cedar St., New York, N. Y. (37). 1891. **I**

LINDSLEY, J. BERRIEN, M.D., Nashville, Tenn. (1). 1874. **F**

Lintner, J. A., N. Y. State Entomologist, Room 27, Capitol, Albany, N. Y. (22). 1874. **F**

Livermore, Wm. R., Maj. of Eng. U. S. A., Care War Department, Washington, D. C. (38). 1895. **C H**

Lloyd, John Url, Pharmaceutical Chemist, Court and Plum Sts., Cincinnati, Ohio (38). 1890. **C F**

Long, Prof. John H., 40 Dearborn St., Chicago, Ill. (41). 1895. **C**

Lord, Prof. Nat. W., State University, Columbus, Ohio (29). 1881. **C**

Loubat, le Duc de, 47 rue Dumont d'Urville, Paris, France (46). 1897. **H**

Loughridge, Dr. R. H., Ass't Prof. Agric. Chem. and Agric. Geol., University of California, Berkeley, Cal. (21). 1874. **E C**

Love, Edward G., 80 E. 55th St., New York, N. Y. (24). 1882. **C**

Low, Seth, President Columbia University, New York, N. Y. (29). 1890.

Lowell, Percival, 53 State St., Boston, Mass. (36). 1896. **A**

Ludlow, Lt. Col. William, Corps of Eng. U. S. A., Army Building, New York, N. Y. (33). 1897. **D B**

Lyford, Edwin F., Springfield, Mass. (33). 1896. **B C H**

Lyle, David Alexander, Captain Ordnance Dept. U. S. A., Ordnance Office, War Department, Washington, D. C. (28). 1880. **D**

Lyon, Dr. Henry, 34 Monument Sq., Charlestown, Mass. (18). 1874.

McCauley, Major C. A. H., Q. M., U. S. A., 1428 Arch St., Philadelphia, Pa. (29). 1881.

McClintock, Emory, Morristown, N. J. (43). 1895. **A**

McCreath, Andrew S., 223 Market St., Harrisburg, Pa. (33). 1889. **C E**

McCurdy, Chas. W., Ph.D., Prof. of Chemistry, University of Idaho, Moscow, Idaho (35). 1895. **F E**

McGee, Dr. Anita Newcomb, Bureau of American Ethnology, Washington, D. C. (37). 1892. **H**

McGee, W J, Bureau of American Ethnology, Washington, D. C. (27). 1882. **H E**

McGill, John T., Ph.D., Vanderbilt Univ., Nashville, Tenn. (36). 1888. **C**

McGregory, Prof. J. F., Colgate Univ., Hamilton, N. Y. (35). 1892. **C**

McMahon, James, Ithaca, N. Y. (36). 1891. **A**

MacMillan, Prof. Conway, University of Minnesota, Minneapolis, Minn. (42). 1894. **G**

McMurtrie, William, 106 Wall St., New York, N. Y. (22). 1874. **C**

McNeill, Malcolm, Lake Forest, Ill. (32). 1885. **A**

McRae, Austin Lee, Consulting Electrical Eng., 306 Oriel Building, St. Louis, Mo. (39). 1891. **B**

Mabery, Prof. C. F., Case School of Applied Science, Cleveland, Ohio (29). 1881. **C**

Macbride, Prof. Thomas H., Iowa City, Iowa (38). 1890. **G**

Macfarlane, Prof. A., Lehigh University, S. Bethlehem, Pa. (34). 1886. **B A**

Macloskie, Prof. George, Princeton University, Princeton, N. J. (25). 1882. **F**

Magie, Prof. Wm. F., Princeton University, Princeton, N. J. (35). 1887.

MANN, B. PICKMAN, 1918 Sunderland Place, Washington, D. C. (22). 1874. **I F**

Marçy, Oliver, LL.D., Evanston, Ill. (10). 1874. **E**

Marlatt, Charles L., 1st Ass't Entomologist, Department of Agriculture, Washington, D. C. (40). 1895. **F**

Marsh, Prof. C. Dwight, Ripon, Wis. (34). 1893. **F E**

MARSH, PROF. O. C., Yale University, New Haven, Conn. (15). 1874. **F H**

Martin, Artemas, U. S. Coast Survey, Washington, D. C. (38). 1890. **A**

Martin, Prof. Daniel S., 75 W. 55th St., New York, N. Y. (23). 1879. **E F**

Martin, Prof. Wm. J., Davidson College, N. C. (31). 1884. **C E**

Marvin, C. F., U. S. Weather Bureau, Washington, D. C. (39). 1892. **B**

Marvin, Frank O., University of Kansas, Lawrence, Kan. (35). 1894. **D**

Mason, Prof. Otis T., National Museum, Washington, D. C. (25). 1877. **H**

Mason, Dr. William P., Prof. Rensselaer Polytechnic Institute, Troy, N. Y. (31). 1886. **C**

Matthews, Dr. Washington, 21 South Ingalls St., Ann Arbor, Mich. (37). 1888. **H**

Meehan, Thomas, Germantown, Pa. (17). 1875 **G**

Mees, Prof. Carl Leo, Rose Polytechnic Institute, Terre Haute, Ind. (24). 1876. **B C**

Mell, Prof. P. H., Polytechnic Institute, Auburn, Ala. (39). 1895. **E G**

Mendenhall, Prof. T. C., President Worcester Polytechnic Institute, Worcester, Mass. (20). 1874. **B**

Menocal, Anicito G., C. E., U. S. N., Navy Yard, Brooklyn, N. Y. (36). 1888. **D**

Mercer, H. C., Doylestown, Bucks Co., Pa. (41). 1893. **H**

Merrill, Frederick J. H., Ph.D., Ass't Director New York State Museum, Albany, N. Y. (35). 1887. **E**

Merriman, C. C., 1910 Surf St., Lake View, Chicago, Ill. (29). 1880. **F**

Merriman, Prof. Mansfield, S. Bethlehem, Pa. (32). 1885. **A D**

Merritt, Ernest, Ithaca, N. Y. (33). 1890. **B**

Metcalf, William, Pittsburgh, Pa. (33). 1894. **D**

Michael, Mrs. Helen Abbott, 44 Mount Vernon St., Boston, Mass. (33). 1885. **C F**

Michelson, Prof. A. A., Chicago University, Chicago, Ill. (26). 1879. **B**

Miles, Prof. Manly, Lansing, Mich. (29). 1890. **F I**

Miller, Prof. William S., University of Wisconsin, Madison, Wis. (42). 1894. **F**

Mills, James, M.A., Guelph, Ontario, Can. (31). 1895. **I C**

Mills, Prof. Wesley, McGill College, Montreal, P.Q., Can. (31). 1886. **F H**

Minot, Dr. Charles Sedgwick, Harvard Medical School, Back Bay, Boston, Mass. (28). 1880. **F**

Minot, Francis, M.D., Readville, Mass. (29). 1884.

Mohr, Dr. Charles, Mobile, Ala. (40). 1895. **G**

Moler, Geo. S., 106 University Ave., Ithaca, N. Y. (38). 1892.

Moody, Robert O., M.D., Fair Haven Heights, New Haven, Conn. (35). 1892. **F**

Mooney, James, Bureau of Ethnology, Washington, D. C. (38). 1890. **H**

Moore, Clarence B., 1321 Locust St., Philadelphia, Pa. (44). 1897. **H**

Moore, E. Hastings, University of Chicago, Chicago, Ill. (39). 1891. **A**

Moore, Prof. J. W., M.D., Lafayette College, Easton, Pa. (22). 1874. **B D A**

Moore, Veranus A., M.D., Ithaca, N. Y. (40). 1892. **F**

Moore, Prof. Willis L., Chief of Weather Bureau, Dep't Agriculture, Washington, D. C. (44). 1897. **B**

Moorehead, Warren K., Ohio State Univ., Columbus, Ohio (38). 1890. **H**

Moreland, Prof. S. T., Lexington, Va. (33). 1894. **B D**

Morley, Prof. Edward W., 23 Cutler St., Cleveland, Ohio (18). 1876. **C B E**

Morse, Prof. E. S., Salem, Mass. (18). 1874. **F H**

Morton, H., Stevens Institute Technology, Hoboken, N. J. (18). 1875. **B C**

Moser, Lieut. Comd'r Jeff. F., U. S. N., Com'dg U. S. F. S. Str. Albatross, Navy Pay Office, San Francisco, Cal. (28). 1889. **E**

Munroe, Prof. C. E., Columbian Univ., Washington, D. C. (22). 1874. **C**

Murdoch, John, Public Library, Boston, Mass. (29). 1886. **F H**

Myers, John A., Agric Exper. Station, Morgantown, W. Va. (30). 1889. **C**

Nagle, Prof. James C., A. and M. College, College Station, Texas (40). 1893. **D B**

Nardroff, Ernest R. von, 360a Tompkins Ave., Brooklyn, N. Y. (44). 1896. **B**

Nef, J. U., University of Chicago, Chicago, Ill. (39). 1891. **C**

Nelson, Prof. A. B., Centre College, Danville, Ky. (30). 1882. **A B D**

Newcomb, Prof. S., Navy Dep't, Washington, D. C. (13). 1874. **A B**

Newcombe, Frederick Chas., 51 E. Liberty St., Ann Arbor, Mich. (43). 1896. **G**

Newell, F. H., U. S. Geological Survey, Washington, D. C. (40). 1893.

Newell, William Wells, Editor Journal American Folk Lore, Cambridge, Mass. (41). 1893. **H**

Nichols, Ernest Fox, Hamilton, N. Y. (41). 1893. **B**
Nichols, E. L., Ph.D., Cornell University, Ithaca, N. Y. (28). 1881. **B C**
Niles, Prof. W. H., Cambridge, Mass. (16). 1874. **E**
NORTON, PROF. THOMAS H., University of Cincinnati, Cincinnati, Ohio (35). 1887. **C**
Novy, Dr. Frederick G., University of Michigan, Ann Arbor, Mich. (36). 1889. **C**
Noyes, Prof. Arthur A., Massachusetts Institute Technology, Back Bay, Boston, Mass. (45). 1897. **C**
Noyes, Miss Mary C., Ph.D., Lake Erie Seminary, Painesville, Ohio (43). 1896. **B**
Noyes, Prof. Wm. A., Rose Polytechnic Institute, Terre Haute, Ind. (32). 1885. **C**
Nuttall, Mrs. Zelia, care Peabody Museum, Cambridge, Mass. (35). 1887. **H**
Nutting, Prof. Charles C., State University of Iowa, Iowa City, Iowa (40) 1892. **F**

Ordway, Prof. John M., Tulane University, New Orleans, La. (9). 1875. **C**
Orleman, Miss Daisy M., M.D., Peekskill Military Acad., Peekskill, N. Y. (40). 1897. **F**
Orr, William, jr. 30 Firglade Ave., Springfield, Mass. (39). 1895. **F B**
Orton, Prof. Edward, President Ohio Agricultural and Mechanical College, Columbus, Ohio (19). 1875. **E**
Osborn, Henry F., Columbia University, New York, N. Y. (29). 1883. **F**
Osborn, Herbert, Ames, Iowa (32). 1884. **F**
Osmond, Prof. I. Thornton, State College, Centre Co., Pa. (33). 1889. **B A C**

Paine, Cyrus F., 806 Granite Building, Rochester, N. Y. (12). 1874. **B A**
Palache, Charles, University Museum, Cambridge, Mass. (44). 1896. **E**
Palfray, Hon. Charles W., Salem, Mass. (21). 1874.
Pammel, Prof. L. H., Iowa Agricultural College, Ames, Iowa (39). 1892.
Parke, John G., General U. S. A., 16 Lafayette Square, Washington, D. C. (29). 1881. **D**
PARKHURST, HENRY M., 173 Gates Ave., Brooklyn, N. Y. (23). 1874. **A**
Parks, C. Wellman, Civil Eng., U. S. N., care Bureau of Navigation, Washington, D. C. (42). 1897.
Parsons, Prof. C. Lathrop, Durham, N. H. (41). 1896.
Patrick, Geo. E., Ames, Iowa (36). 1890. **C**
Patterson, Geo. W., jr., Ann Arbor, Mich. (44). 1896.
Patterson, Harry J., College Park, Prince George's Co., Md. (36). 1890. **C**
Paul, Prof. Henry M., U. S. Naval Observatory, Washington, D. C. (33). 1885. **A B**
Peet, Rev. Stephen D., 5327 Madison Ave., Chicago, Ill. (24). 1881. **H**
Peirce, George James, Stanford University, Cal. (44). 1897. **G**
Penrose, Dr. R. A. F., 1331 Spruce St., Philadelphia, Pa. (38). 1890. **E**
Perkins, Prof. George H., Burlington, Vt. (17). 1882. **H F E**
Perry, Arthur C., 226 Halsey St., Brooklyn, N. Y. (43). 1896. **A B**

Peter, Alfred M., 236 E. Maxwell St., Lexington, Ky. (29). 1890. C
Peters, Edw. T., P. O. Box 265, Washington, D. C. (33). 1889. I
Pettee, Prof. Wm. H., 52 Thompson St., Ann Arbor, Mich. (24). 1875. E
Phillips, Prof. A. W., New Haven, Conn. (24). 1879.
Phillips, Prof. Francis C., Western University, Allegheny, Pa. (36). 1889. C
Phillips, Dr. Wm. A., Evanston, Ill. (41). 1895. H
Pickering, Prof. E. C., Director of Harvard Observatory, Cambridge, Mass. (18). 1875. A B
Pierce, Perry Benj., U. S. Patent Office, Washington, D. C. (40). 1895. H
Pillsbury, Prof. John H., Stoneham, Mass. (23). 1885. F H
Platt, Franklin, Ass't Geologist, 2nd Geological Survey of Pennsylvania, 1617 Chestnut St., Philadelphia, Pa. (27). 1882. E
Pohlman, Dr. Julius, Buffalo, N. Y. (32). 1884. E F
Powell, Major J. W., Washington, D. C. (23). 1875. E H
Power, Frederick B., Ph.D., care Messrs. Burroughs, Wellcome & Co., 42 Snow Hill, London, E. C., England (31). 1887. C
Prentiss, D. Webster, M.D., 1101 14th St., N. W., Washington, D. C. (29). 1882. F
Prentiss, Robert W., Prof. of Mathematics and Astronomy, Rutgers College, New Brunswick, N. J. (40). 1891. A
Prescott, Prof. Albert B., Ann Arbor, Mich. (23). 1875. C
Pritchett, Henry S., Director Observatory, Washington University, St. Louis, Mo. (29). 1881. A
Prosser, Charles S., Prof. of Geology, Union College, Schenectady, N. Y. (33). 1891. E F
Pulsifer, Wm. H., Newton Centre, Mass. (26). 1879. A H
Pupin, Dr. M. I., Columbia University, New York, N. Y. (44). 1896. B
Putnam, Prof. F. W., Curator Peabody Museum American Archæology and Ethnology, Cambridge, Mass.; Curator Dept. Anthropology, Amer. Museum Nat. History, Central Park, New York, N. Y. (Address as Permanent Secretary A. A. A. S., Salem, Mass.) (10). 1874. H
Pynchon, Rev. T. R., Trinity College, Hartford, Conn. (23). 1875.

Rathbun, Richard, Smithsonian Institution, Washington, D. C. (40). 1892. F
Raymond, Rossiter W., 13 Burling Slip, New York, N. Y. (15). 1875. E I
Raymond, Prof. Wm. G., Rensselaer Polytechnic Institute, Troy, N. Y. (44) 1896. D
Rees, Prof. John K., Columbia Univ., New York, N. Y. (26). 1878. A E B
Reese, Charles L., 1801 Linden Ave., Baltimore, Md. (39). 1892. C
Reese, Jacob, 400 Chestnut St., Philadelphia, Pa. (33). 1891. D B
Reid, Harry Fielding, Johns Hopkins Univ., Baltimore, Md. (36). 1893. B
Remsen, Prof. Ira, Johns Hopkins Univ., Baltimore, Md. (22). 1875. C
Rice, Prof. Wm. North, Wesleyan University, Middletown, Conn. (18). 1874. E F
Richards, Prof. Charles B., 137 Edwards St., New Haven, Conn. (33). 1885. D

Richards, Edgar, 341 W. 88th St., New York, N. Y. (31). 1886. **C**

Richards, Prof. Robert H., Massachusetts Institute Technology, Back Bay, Boston, Mass. (22). 1875. **D**

Richards, Mrs. Robert H., Massachusetts Institute of Technology, Back Bay, Boston, Mass. (23). 1878. **C**

Richardson, Clifford, Sup't of Tests, Barber Asphalt Paving Co., Long Island City, N. Y. (30). 1884. **C**

Ricketts, Prof. Palmer C., 17 1st St., Troy, N. Y. (33). 1887. **D A**

Ricketts, Prof. Pierre de Peyster, 104 John St., New York, N. Y. (26). 1880. **C D E**

Ripley, William Z., Ph.D., Newton Centre, Mass. (44). 1897. **H I**

Robinson, Benjamin Lincoln, Curator Harvard Herbarium, Cambridge, Mass. (41). 1893. **G**

Robinson, Prof. Franklin C., Bowdoin College, Brunswick, Me. (29). 1889. **C D**

Robinson, Prof. S. W., 1353 Highland St., Columbus, Ohio (30). 1883. **D B A**

Rockwell, Gen. Alfred P., Manchester, Mass. (10). 1882. **E**

Rockwell, Chas. H., Box 293, Tarrytown, N. Y. (28). 1883. **A D**

Rockwood, Prof. Charles G., jr., Princeton College, Princeton, N. J. (20). 1874. **A E B D**

Rogers, Prof. W. A., Colby University, Waterville, Me. (15). 1875. **A B D**

Rominger, Dr. Carl, Ann Arbor, Mich. (21). 1879. **E**

Rood, Prof. O. N., Columbia University, New York, N. Y. (14). 1875. **B**

Rosa, Edward Bennett, Prof. of Physics, Wesleyan University, Middletown, Conn. (39). 1892. **A B**

Ross, Waldo O., 1 Chestnut St., Boston, Mass. (29). 1882.

Rotch, A. Lawrence, Director of Blue Hill Meteorological Observatory, Hyde Park, Mass. (39). 1896. **B**

Rowland, Prof. Henry A., Baltimore, Md. (29). 1880. **B**

Rowlee, W. W., Cornell University, Ithaca, N. Y. (41). 1894. **G**

Runkle, Prof. J. D., Massachusetts Institute of Technology, Boston, Mass. (2). 1875. **A D**

Rusby, Henry H., M.D., College of Pharmacy, 211 E. 23d St., New York, N. Y. (36). 1890. **G**

Russell, Prof. H. L., University of Wisconsin, Madison, Wis. (41). 1894. **G**

Russell, I. C., University of Mich., Ann Arbor, Mich. (25). 1882. **E**

Ryan, Harris J., Cornell University, Ithaca, N. Y. (38). 1890. **B**

Sadtler, Sam'l P., 1042 Drexel Building, Philadelphia, Pa. (22). 1875. **C**

Safford, Dr. James M., Nashville, Tenn. (6). 1875. **E C F**

Safford, Prof. Truman H., Williamstown, Mass. (41). 1892. **A**

Salisbury, Prof. R. D., Chicago University, Chicago, Ill. (37). 1890. **B E**

Salmon, Daniel E., Department of Agriculture, Washington, D. C. (31). 1885. **F**

Saunders, Prof. Charles E., 32 St. Mary St., Toronto, Ontario, Can. (41). 1895. C

Saunders, William, LL.D., F.R.S.C., F.L.S., Director Canadian Experimental Farms, Ottawa, Can. (17). 1874. F

Saville, Marshall H., American Museum Natural History, Central Park, New York, N. Y. (39). 1892. H

SCHAEBERLE, J. M., Astronomer in the Lick Observatory, San José, Cal. (34). 1886. A

Schanck, Prof. J. Stillwell, Princeton, New Jersey (4). 1882. C B H

Schott, Charles A., U. S. Coast and Geodetic Survey Office, Washington, D. C. (8). 1874. A

Schwarz, E. A., 230 New Jersey Ave., Washington, D. C. (29). 1895. F

Schweinitz, Dr. E. A. de, Department of Agriculture, Washington, D. C. (36). 1889. C

Schweitzer, Prof. Paul, State University of Missouri, Columbia, Mo. (24). 1877. C B

SCUDDER, SAMUEL H., Cambridge, Mass. (13). 1874. F

Scull, Miss S. A., Smethport, McKean Co., Pa. (40). 1895. H

Seaman, W. H., Chemist, 1424 11th St. N. W., Washington, D. C. (28). 1874. C F

Searle, Prof. Geo. M., Catholic University, Washington, D. C. (39). 1891. A

See, Horace, 1 Broadway, New York, N. Y. (34). 1886. D

Seymour, Arthur Bliss, Cambridge, Mass. (36). 1890. G

Seymour, Paul Henry, 479 Second Ave., Detroit, Mich. (44). 1896. C

Sharp, Dr. Clayton H., Ithaca, N. Y. (45). 1897.

Sharples, Stephen P., 13 Broad St., Boston, Mass. (29). 1884. C

Shaw, Prof. James Byrnie, 1030 Grove St., Jacksonville, Ill. (43). 1896. A

Sheldon, Samuel, A.M., Ph.D., Polytechnic Institute, Brooklyn, N. Y. (42). 1894. B

Shelton, Prof. Edward M., Department of Agriculture, Brisbane, Queensland, Australia (32). 1892. F

Shimer, Porter W., E.M., Easton, Pa. (38). 1889. C

Shufeldt, Dr. R. W., Smithsonian Institution, Washington, D. C. (40). 1892. F

Sias, Solomon, M.D., Schoharie, Schoharie Co., N. Y. (10). 1874.

Sigsbee, Chas. D., Com'd'r U. S. N., U. S. Hydrographic Office, Washington, D. C. (28). 1882. D E

Simon, Dr. Wm., 1348 Block St., Baltimore, Md. (29). 1895. C

Simonds, Prof. Frederic W., University of Texas, Austin, Texas (25). 1888. E F

Skilton, James A., 115 Broadway, New York, N. Y. (43). 1895. I

Skinner, Aaron N., U. S. Naval Observatory, Washington, D. C. (40). 1893. A

Smith, Alex., Ph.D., University of Chicago, Chicago, Ill. (40). 1892. C

Smith, Prof. Chas. J., 35 Adelbert St., Cleveland, Ohio (32). 1885. A B

Smith, Prof. Edgar F., University of Pennsylvania, Philadelphia, Pa. (33). 1891. C

Smith, Edwin, Rockville, Montgomery Co., Md. (30). 1882. **A B**

Smith, Prof. Erastus G., Beloit College, Beloit, Wis. (34). 1887. **C**

Smith, Erwin F., Department of Agriculture, Washington, D. C. (34). 1890. **G**

Smith, Prof. Eugene A., University, Ala. (20). 1877. **E C**

Smith, Harlan I., American Museum Natural History, Central Park, New York, N. Y. (41). 1896. **H**

Smith, James Perrin, Ph.D., Ass't Prof. of Paleontology, Leland Stanford Junior University, Palo Alto, Cal. (37). 1894. **C E**

Smith, John B., Professor of Entomology, Rutgers College, New Brunswick, N. J. (32). 1884. **F**

SMITH, QUINTIUS C., M.D., 617 Colorado St., Austin, Texas (26) 1881. **F**

Smock, Prof. John Conover, Trenton, N. J. (23). 1879. **E**

Smyth, C. H., jr., Clinton, N. Y. (38). 1894. **E**

Snow, Prof. F. H., Lawrence, Kan. (29). 1881. **F E**

Snyder, Prof. Harry, Saint Anthony Park. Minn. (44). 1897. **C**

Snyder, Prof. Monroe B., High School Observatory, Philadelphia, Pa. (24). 1882. **A B**

Soule, R. H., care Baldwin Locomotive Works, Philadelphia, Pa. (33). 1886. **D**

Spencer, Prof. J. William, 1320 Corcoran St., Washington, D. C. (28). 1882. **E**

SPENZER, JOHN G., M.D., 370 Central Ave., Cleveland, Ohio (37). 1895. **C**

Spinney, L. B., Iowa State College, Ames, Iowa (42). 1897. **B**

Springer, Dr. Alfred, Box 621, Cincinnati, Ohio (24). 1880. **C**

Squibb, Edward R., M.D., 152 Columbia Heights, Brooklyn, N. Y. (43). 1896.

Starr, Frederick, Ph.D., Prof. University of Chicago, Chicago, Ill. (36). 1892. **H E**

Stearns, Robert E. C., Shaffer House, 525 Sand St., Los Angeles, Cal. (18). 1874. **F**

Stedman, John M., Prof. of Entomology, State University of Missouri, Columbia, Mo. (40). 1892. **F**

Steinmetz, Chas. Proteus, General Electric Co., Schenectady, N. Y. (40). 1895. **B**

Stejneger, Leonhard, Curator Dept. of Reptiles, National Museum, Washington, D. C. (40). 1892. **F**

STEPHENS, W. HUDSON, Lowville, N. Y. (18). 1874. **E H**

Sternberg, George M., M.D., LL.D., Surgeon General U. S. A., War Department, Washington, D. C. (24). 1880. **F**

Stevens, Prof. W. LeConte, Rensselaer Polytechnic Institute, Troy, N. Y. (29). 1882. **B**

Stevenson, Mrs. Cornelius, 237 S. 21st St., Philadelphia, Pa. (33). 1895. **H**

Stevenson, Prof. John J., Univ. Heights, New York, N. Y. (36). 1888. **E**

Stevenson, Mrs. Matilda C., Bureau of Ethnology, Washington, D. C. (41). 1893. **H**

Stieglitz, Dr. Julius, University of Chicago, Chicago, Ill. (39). 1895. **C**

Stokes, Henry Newlin, Ph.D., U. S. Geological Survey, Washington, D. C. (38). 1891. **C E**

Stone, Ormond, Director Leander McCormick Observatory, University of Virginia, Va. (24). 1876. **A**

Story, Prof. Wm. E., Clark University, Worcester, Mass. (29). 1881. **A**

Stowell, Prof. T. B., Potsdam, N. Y. (28). 1885. **F**

Stuart, Prof. A. P. S., Lincoln, Nebraska (21). 1874. **C**

Sturgis, Wm. C., 384 Whitney Ave., New Haven, Conn. (40). 1892. **G**

Sturtevant, E. Lewis, M.D., S. Framingham, Mass. (29). 1882. **G**

Swingle, Walter T., Department of Agriculture, Washington, D. C. (40). 1892. **G**

Tainter, Charles Sumner, Central Power Station, Washington, D. C. (29). 1881. **B D A**

Talbot, Henry P., Prof. Analytical Chemistry, Massachusetts Institute of Technology, Back Bay, Boston, Mass. (44). 1896. **C**

Taylor, Frank B., 391 Fairfield Ave., Fort Wayne, Ind. (39). 1897.

Tesla, Nikola, LL.D., 55 W. 27th St., New York, N. Y. (43). 1895. **B**

Thomas, Benjamin F., Ph.D., State University, Columbus, Ohio (29). 1882. **B A**

Thomas, Prof. M. B., Crawfordsville, Ind. (41). 1894. **G**

Thompson, Joseph Osgood, Amherst, Mass. (41). 1893.

Thomson, Elihu, Thomson-Houston Electric Co., Lynn, Mass. (37). 1888. **B**

Thomson, Wm., M.D., 1426 Walnut St., Philadelphia, Pa. (33). 1885. **B**

Thornburg, Charles L., Prof. Math. and Astron., Lehigh University, S. Bethlehem, Pa. (44). 1897. **A**

Thruston, Gates Phillips, Nashville, Tenn. (38). 1890. **H**

Thruston, R. C. Ballard, care Ballard & Ballard Co., Louisville, Ky. (36). 1896. **E**

Thurston, Prof. R. H., Sibley College, Cornell University, Ithaca, N. Y. (23). 1875. **D**

Tittmann, Otto H., U. S. Coast and Geodetic Survey Office, Washington, D. C. (24). 1888. **A**

Todd, Prof. David P., Director Lawrence Observatory, Amherst College, Amherst, Mass. (27). 1881. **A B D**

Todd, Prof. James E., Box 22, Vermillion, S. Dak. (22). 1886. **E F**

Tooker, William Wallace, Sag Harbor, N. Y. (43). 1895. **H**

Tracy, Sam'l M., Agricultural College, Miss. (27). 1881. **G**

Trelease, Dr. Wm., Director Missouri Botanical Gardens, St. Louis, Mo. (39). 1891. **G**

Trenholm, Hon. W. L., President American Surety Co., 160 Broadway, New York, N. Y. (35). 1896.

Trimble, Prof. Henry, 145 N. 10th St., Philadelphia, Pa. (34). 1889. **C**

True, Fred W., National Museum, Washington, D. C. (28). 1882. **F**

Tucker, Willis G., M.D., Albany Med. College, Albany, N.Y. (29). 1888. **C**

TUCKERMAN, ALFRED, Ph.D., 342 W. 57th St., New York, N. Y. (39). 1891. **C**

Twitchell, E., 10 Bellevue Ave., Mt. Auburn, Cincinnati, Ohio (89). 1891. **C**

Uhler, Philip R., 254 W. Hoffman St., Baltimore, Md. (19). 1874. **F E**

Underwood, Lucien M., Prof. of Botany, Columbia University, New York, N. Y. (33). 1885. **G**

Upham, Warren, Librarian of the Minnesota Historical Society, St. Paul, Minn. (25). 1880. **E**

Upton, Winslow, Brown University, Providence, R. I. (29). 1883. **A**

Van Dyck, Prof. Francis Cuyler, New Brunswick, N. J. (28). 1882. **B C F**

Van Hise, Charles R., University of Wisconsin, Madison, Wis. (37). 1890.

Van Vleck, Prof. John M., Wesleyan University, Middletown, Conn. (28). 1875. **A**

Veeder, Major Albert, M.D., Lyons, Wayne Co., N. Y. (36). 1895.

Venable, Prof. F. P., Chapel Hill, N. C. (39). 1891. **C**

Vogdes, A. W., Captain 5th Artillery, Fort Mason, San Francisco, Cal. (32). 1885. **E F**

Voorhees, Louis A., Agricultural Experiment Station, New Brunswick, N. J. (43). 1895. **C**

Wadsworth, Prof. M. Edward, Ph.D., Director of the Michigan Mining School, State Geologist of Michigan, Houghton, Mich. (23). 1874. **E**

Wagner, Frank C., Rose Polytechnic Institute, Terre Haute, Ind. (34). 1897. **D**

Waite, M. B., Dep't of Agriculture, Washington, D. C. (37). 1893. **G**

Walcott, Charles D., Director U. S. Geological Survey, Washington, D. C. (25). 1882. **E F**

Waldo, Leonard, S. D., 57 Coleman St., Bridgeport, Conn. (28). 1880. **A**

Wallace, Wm., Ansonia, Conn. (28). 1882.

WALLER, E., School of Mines, Columbia University, New York, N. Y. (23). 1874.

Walmsley, W. H., 134-136 Wabash Ave., Chicago, Ill. (28). 1883. **F**

Ward, Prof. Henry A., Rochester, N. Y. (13). 1875. **F E H**

Ward, Lester F., U. S. Geological Survey, Washington, D. C. (26). 1879. **E G**

Ward, Dr. R. H., 53 Fourth St., Troy, N. Y. (17). 1874. **G F**

Ward, Wm. E., Port Chester, N. Y. (36). 1889. **D**

Warder, Prof. Robert B., Howard University, Washington, D. C. (19). 1881. **C B**

WARNER, JAMES D., 199 Baltic St., Brooklyn, N. Y. (18). 1874. **A B**

Warner, Worcester R., 1722 Euclid Ave., Cleveland, Ohio (33). 1888. **A B D**

Warren, Dr. Joseph W., Bryn Mawr Coll., Bryn Mawr, Pa. (31). 1886. **F**

Warren, Prof. S. Edward, Newton, Mass. (17). 1875. **A-I**

Washington, Dr. Henry S., Locust, N. J. (44). 1897. **E**

WATSON, PROF. WM., 107 Marlborough St., Boston, Mass. (12). 1884. **A**

Webb, Prof. J. Burkitt, Stevens Inst., Hoboken, N. J. (31). 1883. **D B A**

Weber, Prof. Henry A., Ohio State Univ., Columbus, Ohio (35). 1888. **F**

Webster, F. M., Wooster, Ohio (35). 1890. **F**

Webster, Prof. N. B., Vineland, N. J. (7). 1874. **B C E**

Weed, Clarence M., Durham, N. H. (38). 1890. **F**

Weld, Prof. Laenas G., State University of Iowa, Iowa City, Iowa (41). 1895. **A**

West, Dr. Charles E., Brooklyn, N. Y. (1). 1895.

White, Prof. H. C., University of Georgia, Athens, Ga. (29). 1885. **C**

White. Prof. I. C., University of West Virginia, Morgantown, W. Va. (25). 1882. **E**

Whiteaves, J. F., Geological Survey, Ottawa, Ontario, Can. (31). 1887. **E F**

Whitfield, J. Edward, 406 Locust St., Philadelphia, Pa. (44). 1896. **C**

Whitfield, R. P., American Museum Natural History, 77th St. and 8th Avenue, New York, N. Y. (18). 1874. **E F H**

Whiting, Miss Sarah F., Wellesley College, Wellesley, Mass. (31). 1883. **B A**

Whitman, Prof. Frank P., Adelbert College, Cleveland, Ohio (33). 1885. **A B**

Wiley, Prof. Harvey W., Department of Agriculture, Washington, D.C. (21). 1874. **C**

Williams, Benezette, 171 La Salle St., Chicago, Ill. (33). 1887. **D**

Williams, Charles H., M.D., 15 Arlington St., Boston, Mass. (22). 1874.

Williams, Prof. Edw. H., jr., 117 Church St., Bethlehem, Pa. (25). 1894. **E D**

Williams, Francis H., M.D., 505 Beacon St., Boston, Mass. (29). 1890.

Williams, Prof. Henry Shaler, Yale University, New Haven, Conn. (18). 1882. **E F**

Williams, Prof. Thomas A., Division of Agrostology, Department of Agriculture, Washington, D. C. (42). 1894. **G**

Willis, Bailey, U. S. Geological Survey, Washington, D. C. (36). 1890.

Willoughby, Charles C., Peabody Museum, Cambridge, Mass. (45). 1897. **H**

Willson, Prof. Frederick N., Princeton, N. J. (33). 1887. **A D**

Willson, Robert W., Cambridge, Mass. (30). 1890. **B A**

Wilson, Joseph M., Room 1036, Drexel Building, Philadelphia, Pa. (33). 1886. **D**

Wilson, Robert N., Macleod, Alberta, Can. (42). 1895. **H**

Wilson, Thomas, National Museum, Washington, D. C. (36). 1888. **H**

Wilson, Prof. William Powell, Department of Biology, University of Pennsylvania, Philadelphia, Pa. (38). 1889. **G**

Winchell, Horace V., 1306 S. E. 7th St., Minneapolis, Minn. (34). 1890. **E C**

Winchell, Prof. N. H., University of Minnesota, Minneapolis, Minn. (19). 1874. **E H**

Winterhalter, A. G., Lt. U. S. N., care Navy Department, Washington, D. C. (37). 1893. **A**

Withers, Prof. W. A., Agricultural and Mechanical College, Raleigh, N. C. (38). 1891. **C**

Wolff, Dr. J. E., 15 Story St., Cambridge, Mass. (36). 1894. **E**

Woll, Fritz Wilhelm, Madison, Wis. (42). 1897. **C**

Woodbury, C. J. H., Amer. Bell Telephone Co., 125 Milk St., Boston, Mass. (29). 1884. **D**

Woodman, Dr. Durand, 80 Beaver St , New York, N. Y. (41). 1896.

Woodrow, James, President South Carolina College, Columbia, S. C. (43). 1895. **E**

Woods, Albert F., Dep't of Agriculture, Washington, D. C. (43) 1897.

Woodward, R. S., Columbia University, New York, N. Y. (33). 1885. **ABD**

Worthen, W. E., 63 Bleeker St., New York, N. Y. (36). 1888. **D**

Wrampelmeier, Theo. J., Room 17, Appraiser's Building, San Francisco, Cal. (34). 1887. **C**

Wright, Prof. Albert A., Oberlin College, Oberlin, Ohio (24). 1880. **E F**

Wright, Prof. Arthur W., Yale University, New Haven. Conn. (14). 1874. **A B**

Wright, Carroll D., LL.D., Department of Labor, Washington, D. C. 1894. **I**

Wright, Rev. Geo. F., Oberlin College, Oberlin, Ohio (29). 1882. **E H**

Würtele, Rev. Louis C., Acton Vale, P. Q., Can. (11). 1875. **E**

Youmans, Wm. Jay, M.D., Popular Science Monthly, 72 Fifth Ave., New York, N. Y. (28). 1889. **F C**

Young, A. V. E., Northwestern University, Evanston, Ill. (33). 1886. **C B**

Young, C. A., Prof. of Astronomy, Princeton University, Princeton, N. J. (18). 1874. **A B D**

Zalinski, E. L., U. S. A., Century Club, 7 W. 43d St., New York, N. Y. (Temporary address care U. S. Legation, Tokio, Japan.) (36). 1891. **D**

Ziwet, Alexander, 44 Madison St., Ann Arbor, Mich. (38). 1890. **A**

[763 HONORARY FELLOWS AND FELLOWS.]

SUMMARY.—PATRONS, 2; CORRESPONDING MEMBER, 1; MEMBERS, 844; HONORARY FELLOWS, 3; FELLOWS, 760.

DECEMBER 31, 1897, TOTAL NUMBER OF MEMBERS OF THE ASSOCIATION, 1610.

DECEASED MEMBERS.

A list of deceased members of the Association, so far as known at the time of publishing the volume of Proceedings of the Springfield meeting, May 1896, is given in that volume. At the Buffalo meeting the Council directed the Permanent Secretary to omit the printing of the full list of deceased members in the annual volumes and to print only the additions to the list.

Since the publication of the list printed in the Buffalo volume (45), notices have been received of the decease of the following members. This list also includes corrections of the lists printed in the Springfield and Buffalo volumes.

David Leonard Barnes, Chicago, Ill. (43). Born in Smithfield, R. I., Aug. 23, 1858. Died in New York, N. Y., Dec. 15, 1896.

David Beveridge, Des Moines, Iowa (33). Born in Falkirk, Scotland, Apr. 3, 1841. Died on Steamship Anchoria, mid Atlantic, Feb. 7, 1897.

William Dorr Boardman, Roxbury, Mass. (38). Born in Roxbury, Mass. Dec. 27, 1869. Died in Kissengen, Bavaria, Sept. 4, 1896.

Alvan G. Clark, Cambridgeport, Mass. (28). Born in Fall River, Mass., June 10, 1832. Died in Cambridge, Mass., June 9, 1897.

Edward Drinker Cope, Philadelphia, Pa. (17). Born in Philadelphia, Pa., July 28, 1840. Died in Philadelphia, Pa., April 12, 1897.

George W. Dean, Fall River, Mass. (15). Died in Fall River, Mass., Jan. 23, 1897.

James R. Eaton, Liberty, Mo. (29). Died in Cairo, Egypt, Feb., 1897.

Lucius Fairchild, Madison, Wis. (42). Born in Franklin Mills (now Kent), Ohio, Dec. 27, 1831. Died in Madison, Wis., May 23, 1896.

Edward Hubbard Fitch, Jefferson, Ohio (11). Born May 27, 1837. Died in Conneaut, Ohio, Sept. 9, 1897.

Traill Green, Easton, Pa. (1). (Founder.) Died in Easton, Pa., April 29, 1897, aged 84.

Asa Horr, Dubuque, Iowa (21). Died in June, 1896.

Gardiner Greene Hubbard, Washington, D. C. (40). Born in Boston, Mass., Aug. 25, 1822. Died in Washington, D. C., Dec. 11, 1897.

Joseph F. James, Hingham, Mass. (30). Died in Hingham, Mass., March 29, 1897.

James M. Leete, St. Louis, Mo. (27). Born in De Reuter, N. Y., Feb. 10, 1833. Died at Mineral Point, Wis., April 17, 1897.

Alfred M. Mayer, Hoboken, N. J. (19). Born in Baltimore, Md., in 1836. Died in South Orange, N. J, July 13, 1897.

John G. Morris, Baltimore, Md. (12). Born in York, Pa., Nov. 14, 1803. Died in Lutherville, Md., Oct. 10, 1895.

William R. Pedrick, Lawrence, Mass. (22). Died in Lawrence, Mass. Sept., 1897.

E. S. Ritchie, Newton Highlands, Mass. (10). Born Aug. 18, 1814. Died June 1, 1895.

George J. Wardwell. Rutland, Vt. (20). Born in Rumford, Me., Sept. 24, 1827. Died in Rutland, Vt., Dec. 18, 1895.

Joseph D. Weeks. Pittsburgh, Pa. (35). Born in Lowell, Mass., Dec. 3, 1840. Died in Pittsburgh, Pa., Dec. 26, 1896.

W. C. Winlock, Washington, D. C. (33). Died at Bay Head, N. J., Sept. 20, 1896, aged 37.

De Volson Wood, Hoboken, N. J. (29).

T. G. Wormley, Philadelphia, Pa. (20). Died Jan. 3, 1897.

(80)

ADDRESS

BY

THEODORE GILL,

THE RETIRING PRESIDENT.[1]

EDWARD DRINKER COPE, NATURALIST — A CHAPTER IN THE HISTORY OF SCIENCE.

I.

Bitter constraint, and sad occasion dear,
Compels me to disturb your season due:
For Lycidas is dead, dead ere his time,
Our Lycidas, and hath not left his peer.

On the morning of the 13th of April, in a car on my way from a funeral in New York to Washington, a newspaper notice of the death, the day before, of my old friend, E. D. Cope, caught my eye. Shocked by the intelligence, I dropped the paper, and memory recalled various incidents of our long acquaintance.

The threnody of Milton,[2] in commemoration of his friend Edward King, also rose to recollection, and the lines just quoted seemed to me to be peculiarly fitted for the great man just dead. He was, indeed, no longer young and had attained his prime,[3] but he had planned work for many years to come and had well advanced in the execution of some of it. He had truly died before his time and

[1] Professor GILL, the Senior Vice President of the Association for the Buffalo meeting, became President of the Association on the death of President COPE. At the meeting of the Council on April 21, 1897, President GILL was requested to prepare an address to take the place of the one which would have been delivered by President COPE had his life been spared. At a meeting of the Council on August 13 it was voted that Professor Gill's name be included in the list of Past Presidents of the Association.—PERMANENT SECRETARY.

[2] Milton, Poems. XVII.

[3] In the extract from Milton's poem, *time* has been substituted for *prime* and *our* for *young*.

had left no peer; the greatest of the long line of American naturalists was prematurely snatched from science and from friends.

My acquaintance with Cope began in 1859. While looking through the part of the Proceedings of the Academy of Natural Sciences of Philadelphia for the month of April, in which my first paper published by the Academy had appeared, I found one by E. D. Cope "On the primary divisions of the Salamandridæ." It seems that the papers by Cope and myself had been passed on by the Committee on Publications on the very same day (April 26th) and appeared in print in juxtaposition. I had not previously heard of the new devotee of science and read his article with as much interest as my own. A well-equipped man had evidently come upon the field and this was the first of the numerous articles that were destined to appear in an uninterrupted flow for nearly four decades. A few months afterwards I met the author in Philadelphia at the Academy. A young man, nineteen years old, about five feet nine or ten inches high, with head carried somewhat backwards and of rather robust frame, stood before me; he had an alert, energetic manner, a pronounced, positive voice, and appeared to be well able to take his part in any trouble. His knowledge was by no means confined to herpetology, but covered a wide range of science, and his preliminary education had been good. We afterwards met from time to time in Philadelphia and Washington and found we had many sympathies in common and some differences.

In one of our first interviews we had quite an argument on the nature of the family group in zoölogy, resulting from criticisms I made on the extended scope he had given to that category in the classification of the Salamanders. Another controversy, I remember, had reference to the vertebral theory of the skull. In an article on the venomous serpents, published in the Proceedings of the Academy for 1859, he had defined the group in terms involving the adoption of that theory, and I ventured to dissent from its reality. I had myself been much impressed with it in former days and, when sixteen years old, had copied in colors an illustration of Owen's so-called archetype reproduced in Carpenter's Physiology. Subsequently, however, the fact that there was only an approximation to the realization of it in the most specialized of fishes and not at all among the lower or higher vertebrates, with other considerations, turned me from it, and I gave my reasons for dissent to Cope. Ultimately he admitted the force of the argument and also abandoned the theory at one time so popular in England and America.

Our acquaintance, thus begun in 1859, continued uninterruptedly till death divided us. We rarely met, indeed, that we did not express difference of opinion respecting some subject, but the difference was never of a serious nature and generally little more than sufficient to enliven intercourse.

II.[1]

The future naturalist was born in Philadelphia on the 28th of July, 1840, and the name Edward Drinker was given to him. He was the descendant of a prosperous line long established in Pennsylvania. His father, Alfred, was a man of cultivated literary taste and did much to train his son's mind in early youth. He had retired from active business and lived in luxurious ease near Germantown,[2] a suburb of Philadelphia. There he had formed an arboretum containing most of the American trees which would thrive in the climate of that region. Amidst such surroundings the youthful Cope grew up.

An active and intelligent interest in Nature became manifest at a very early age. When only about seven years old, during a sea voyage to Boston with his father, the boy is said to have kept a journal which he filled with drawings of "jelly fish, grampuses and other natural objects seen by the way." When eight and a half years old he made his first visit to the Museum of the Academy of Natural Sciences of his native city; this visit was on the "21st day of the 10th Month, 1848," as entered in his journal. He brought away careful drawings, measurements and descriptions of several large birds, as well as of the skeleton of an Ichthyosaurus. His drawing of the fossil reptile bears the explanatory legend in Quaker style: "two of the sclerotic plates look at the eye — thee will see these in it."

At the age of ten he was taken upon a voyage to the West Indies.[3] What were the impressions he derived from that voyage we have not been told. But what has been communicated amply

[1] I am indebted to a brother-in-law of Professor Cope, Mr. Philip C. Garrett, for fuller information and rectifications of statements made in the original address which I have utilized in this edition in the form of notes.

[2] According to Mr. Garrett, in strict accuracy his father either had not retired from active business or had never been in it, having been and remaining what is called an active partner of H. & A. Cope, though it must be admitted, a rather inactive one at all times through very poor health. The home in which Edward was reared from early boyhood was not in Germantown but about a mile east of it on the York road.

[3] Osborn in Science, N. S., V, 706.

justified Professor Osborn in his declaration that "the principal impression he gave in boyhood was of incessant activity in mind and body, reaching in every direction for knowledge, and of great independence in character and action." His school education was mostly carried on in the Westtown Academy,[1] a Quaker institution about twenty-three miles west of Philadelphia. One of his instructors was Dr. Joseph Thomas, a well-known literary worker of Philadelphia and future author of a "Universal Pronouncing Dictionary of Biography and Mythology" (1870), and said to have been an "excellent linguist." Under his guidance Cope obtained a passing knowledge of Latin and Greek. He appears to have had no instruction in any biological science and had no regular collegiate training. He did, however, enjoy the advantage of "a year's study (1858–9) of anatomy and clinical instruction at the University of Pennsylvania," in which the illustrious Leidy was professor of anatomy. But, in the words of his literary executor (Professor H. F. Osborn), "it is evident that he owed far more to paternal guidance in the direct study of nature and to his own impulses as a young investigator than to the five or six years of formal education which he received at school." He was especially fond of map drawing and of geographical studies.

While a school boy, he relieved his studies of the classics and the regular course in which boys of his own age were drilled, by excursions into the fields and woods. Reptile life especially interested him, and he sought salamanders, snakes and tortoises under rocks, stones, fallen trees and layers of leaves, as well as in the ponds and streams of his vicinage. The trophies of his excursions were identified from descriptions in the works in which they were treated, as well as by comparison with identified specimens in the museum of the Academy. He early, and almost without guidance, learned to use the library and collection of the Academy, although he did not become a member until he came of age in 1861.

Cope's first contribution to the Proceedings of the Academy appeared in the part covering April and was, "On the Primary Divisions of the Salamandridæ, with descriptions of the New Species."[2] In this maiden paper he instituted important modifications of the

[1] Mr. Garrett informs me that Cope's "education appears to have been received at home until 1851; then for two years at the Friends' Select School in Philadelphia; from 1853 to 1856 at West Town and from 1856 to 1859 by private tuition and then again at the Select School in Philadelphia."

[2] Proc. Acad. Nat. Sci. Phila., 1859, pp. 122–128.

systems previously adopted in the United States. He soon afterwards catalogued the serpents preserved in the museum of the Academy of Natural Sciences and likewise improved upon the systems previously in vogue. He continued with various papers, describing new species and giving synopses or brief monographs of sundry genera of lizards and anurous amphibians.

For five years his publication was confined almost exclusively to the reptiles and amphibians. (The continuity was only interrupted once in 1862, when he described a new shrew caught by himself in New Hampshire.)[1] Not until 1864 did he begin to extend his field. In that year he described various fishes and a supposed new whale, and gave his first contribution to paleontology in the description of the stegosaurian amphibian called *Amphibamus grandiceps*. But although his attention had become thus divided, he never lost his interest in herpetology and continued to the end of his life to devote much attention to that department. His studies extended to every branch of the subject, covering not only specific details and general taxonomy, but also the consideration of anatomical details, the modifications of different organs, geographical distribution, chronological sequence, genetic relations and physiological consequences. So numerous were his memoirs, so entirely did he cover the field of herpetology, and so marked an impression did he make on the science, that he was well entitled to apply to himself the boast of the Vergilian hero, *Pars magna fui*.

In his earliest essays he manifested the independence and critical spirit which were so characteristic of him later. One knowing all the circumstances of the case may be amused in coming across a passage expressed in the tones of a veteran, published by him when twenty years old: "In proposing the name Zaocys . . . we are giving expression to an opinion *long held by us* as to the unnatural association of species in the so-called genus *Coryphodon* . . . In it we find cylindrical terrestrial species united with compressed subarboricole species, upon a peculiarity whose value as an index of nature appears to us entirely imaginary. The very nature of the *coryphodontian* type of dentition, as distinguished from the *isodontian* and *syncranterian*, would lead us to infer its incon-

[1] An unentitled communication upon certain cyprinoid fish in Pennsylvania was published in the Proceedings of the Academy of Natural Sciences early in 1862 (Proc. 1861, p. 522–524): it is not included in the list of Cope's papers in the catalogue by the Royal Society.

stancy;" and so on.[1] Bold as was the criticism of such herpetologists as Duméril, Bibron and Günther, it was justified by the facts, and the young author's conclusions have received the endorsement of the best succeeding herpetologists, including even the latest author criticised.

In 1863 he paid a visit to Europe, partly for the benefit of his health which had suffered from overwork, and partly for the purpose of seeing the great museums of England, France, Holland, Austria and Prussia. Notwithstanding his ailments, he made good use of his time abroad and systematically examined the collections of reptiles in the chief centers of science. He did not even restrict his studies to herpetology, but extended them to various other subjects.

On his return from Europe, in 1864, he was appointed professor of natural science in Haverford College, an institution chiefly supported by Quakers, but retained the position only three years. During this time, in 1865, he married Miss Annie, daughter of Mr. Andrew Pim, of Chester county, Pa.

In and after 1864, too, he enlarged the range of his studies and publications and also extended them to ichthyology, mammalogy and paleontology. He had always been interested in the philosophical aspects of science and early adopted the conception of descent with modifications to account for the variations of animals and the differentiation into species and higher groups and, in 1869, began to give expression to his peculiar views.

On the death of his father[2] he became heir to a considerable fortune. Part of this was invested in mines which, for a short time, gave promise of good returns; but, it is said, the majority of the stock was held by others and, owing to the incapacity of superintendents and the operations of the controlling stockholders, he lost his interests. While in the enjoyment of his fortune he spent large amounts in collections and personally conducted or sent out expeditions to various places. One of the most important was sent to South America. He filled a large house from cellar to topmost story with his collections and resided in an adjoining one.

In 1871 he conducted an expedition to Kansas and especially investigated the Cretaceous beds of that State and collected their fossils. In 1872 and 1873 he became connected with the U. S.

[1] Proc. Acad. Nat. Sci. Phila., 1860, p. 563.

[2] Cope's father died Dec. 4, 1875.

Geological Survey and for the fossils visited Wyoming in the former year and Colorado in the latter. In 1874 he joined the survey under the command of Lieutenant Wheeler, of the Engineers, and explored New Mexico.

The collections made during these expeditions were large and the unwearied industry and energy, as well as cares, of Cope were rewarded with many well-preserved fossils. These were described in many communications to the Academy of Natural Sciences and the American Philosophical Society and, later, in large volumes published by the general government as reports of the respective surveys with which he was connected.

The various investigations thus opened were continued through the succeeding years. His collections continued to grow in spite of reduced means. He refused even to sell portions for which he was offered liberal sums and, at the cost of personal discomfort, held on to them and made his home for much of the time, in the midst of them, having sold his residential house but kept his museum.

In 1878 he purchased the rights of the proprietors of the American Naturalist and removed it to Philadelphia. Professor Packard, one of the original proprietors, coöperated with him in the editing of it for some years, and he was also assisted by various eminent specialists. In this journal numerous articles of all kinds, including reviews and editorial comments, were published by him. His last words appeared in numbers issued after his death, the leading article in the number for June having been written shortly before his death; it treats of the remarkable mammals of South America, known as Toxodontia.

In 1886 he received an appointment to a chair in the University of Pennsylvania and became professor of geology and paleontology. Such a man naturally awakened the interest of apt pupils, and he was a facile and entertaining lecturer. From the stores of a rich memory he could improvise a discourse on almost any topic within the range of his varied studies. His views were so much in advance of those in any text-book that, for his own convenience, no less than for the benefit of his pupils, he felt compelled to prepare a "Syllabus of lectures on geology and paleontology," but only "Part III, Paleontology of the Vertebrata," was published. It appeared in 1891 and is still a valuable epitome of the classification of the vertebrates, recent as well as fossil, giving in dichotomous

tables the essential characters of all the groups above families and also the names of all the families. His own industry and investigations did much to render this antiquated in even six years and a new edition or work became necessary. "Upon the Tuesday preceding his death he sent to the press an elaborate outline of his University lectures containing his latest ideas of the classification of the Vertebrata."[1]

The enormous mass of publications constantly flowing from his own pen might lead one unacquainted with the author to suppose that he was probably a recluse, but there were few men of his intellectuality who were less disposed to seclude themselves. He enjoyed and gave enjoyment to intellectual company and was a brilliant conversationalist. He was especially fond of academical meetings and was an unusually frequent attendant at the meetings of the American Association as well as of the National Academy of Science. His election to the Presidency of the American Association was highly esteemed by him and doubtless his address would have been a notable one.

In February (1897), Cope's health became seriously affected by nephritic disorder, which, it is said, "might possibly have been remedied by a surgical operation," but to this he would not submit.[2] Notwithstanding failing health, he continued active almost to the last. Finally, the insidious disease invaded his entire system and he died on the 12th of April, in the room he had long used as a study, surrounded by the objects of his life-long attentions.

Such were the chief episodes of Cope's individual life; the facts known are few and the record belongs rather to his family than to us. But Cope's real life was in his work and to the consideration of that work we may now proceed. Let us adopt the order in which he took up the subjects of his investigations and successively look into his contributions to herpetology (III), ichthyology (IV), mammalogy (V) and paleontology (VI); we may then examine his philosophical views and especially those relating to evolution (VII); finally, we may attempt to forecast the position he is destined to enjoy in the history of science (VIII). To know him as he was we must recognize his weakness as well as his

[1] Osborn in Science, May 7, p. 705.

[2] According to Mr. Garrett, "as regards the disorder of which he died it was cystic, not nethritic, the postmortem showing little disease of the kidneys. The surgical operation he intended to undergo, but became too ill before it was accomplished."

strength. He himself has wished this and has asked in the spirit of the Moor:

> Speak of me as I am; nothing extenuate,
> Nor set down aught in malice.

III.

The extent of Cope's contributions to herpetology has been referred to. Herpetology was his first love and continued to be the favorite branch of science to his life's end. His impress on it was, in some respects at least, greater than on any other of the sciences he cultivated, and doubtless the systems he introduced, with some modifications, will be the most lasting. He found herpetology an art; he left it a science: he found it a device mainly for the naming of specimens; he left it the expression of the coördination of all structural features. The reformations he effected in the classification of the anurous amphibians and the saurian reptiles were especially notable.

The anurans had been chiefly differentiated in groups on account of the most superficial characters. Such were the modes of fixation of the tongue or its absence, the development of disk like expansions of the tips of the toes or simply attenuated toes, and the presence or absence of teeth in a jaw. Cope proceeded to investigate the group in an anatomical manner and reached entirely new conclusions. He found that important differences existed in the structure of the sternum, and especially in the connection of the lateral halves. In the common toads and tree toads of Europe and North America, the so-called clavicle and coracoid of each side are "connected by a longitudinal arched cartilage which overlaps that of the opposite side;" while, in the common frogs, the clavicles and coracoids of both sides are connected by a single median cartilage. The former type is now known as the arciferous and the latter as the firmisternal. Although Cope was the first to appreciate the significance of those characters, he did not at once fully realize their morphological value, the name Arcifera having been originally applied by him only to types of that group having teeth. Ultimately he did so, and his views have stood the test of time and the latest critical investigations. He also found that the characters so revealed served to fix the places in the system of the groups in question. In their early stages the Firmisternials (or frogs and their relations) have the shoulder-girdle movable, and thus resemble the Arcifers (toads, etc.), which have the opposite

halves movable during their whole lifetime; thus it became evident that the latter are the lowest or most generalized forms, and the former more advanced and higher in the system. The development of teeth, which had been supposed by the earlier systematists to be of paramount value, and which Cope, following in their footsteps, had also originally unduly valued, has been found to be of quite subordinate importance.

The lizards were also in former times distributed into families and other groups on account of variations in superficial or external characters, such as the form of the tongue, the arrangement of the scales and the development of legs and feet. Cope dissected examples of all the types he could obtain and found that such superficial characters were often misleading, and he proceeded to arrange them with reference to the preponderance of all characters. The structure of the cranium especially was analyzed, and the variations and concordances in the development of various bones were tabulated. These characters were supplemented by others derived from the vertebræ, the shoulder girdle, the teeth, the tongue and the pholidosis. Familiarity with his subject enabled him almost instinctively to assess the relative values of the different characters, and he obtained fitting equations which resulted in a system which has received the approbation of the most competent judges to the present time.

The extent of Cope's influence on herpetology may be to some extent inferred from the catalogues of the richest collection of reptiles and amphibians in existence — the British Museum's. Descriptive catalogues of both the Anurans and Saurians have been published at different times. In the early catalogues are adopted the views current at the dates of publication — 1845 for the lizards; 1858 for the batrachians. New editions were published many years later and the systems of Cope were adopted with slight modifications. In his catalogue of the *Batrachia salientia* Mr. Boulenger, the author, remarked that it appeared "undeniable that the principles of classification laid down by Mr. Cope are more in accordance with the natural affinities of the genera of tailless Batrachians than those employed by other authors; this is amply proved by all we know of their geographical distribution, development and physiology."

In an article[1] published in advance of his catalogue of the

[1] Synopsis of the families of existing Lacertilia. Ann. and Mag. Nat. Hist. (5), XIV, 117.

lizards, Boulenger states that the old classifications are, "on the whole, as unnatural as can be" and that, "like Cope, whose lizard families I regard as the most natural hitherto proposed, I shall lay greater stress on osteological characters and on the structure of the tongue."

It was a long time, however, before Cope's views became popular. Even anatomists of repute refused to follow him. One[1] of them, for example, admitted that "skeletal characters are, indeed, most valuable ones in leading us to detect the deepest and truest affinities of vertebrate animals, but [he urged] these affinities once found, it is very desirable that zoölogical classification should not, if it can possibly be avoided, *repose* upon them only, but rather on more external and more readily ascertainable characters." He, therefore, ventured "to propose a classification derived from that of Dr. Günther."

Cope replied[2] by a fierce review of the work of Dr. Günther, and concluded with the utterance that such views "will only interfere with the progress of knowledge if sincerely held and believed."

But such views were evidently sincerely believed and they did retard the progress of science. An eminent Russian herpetologist objected to the use of anatomical characters. He especially protested against those employed by Boulenger after Cope to the grouping of the lizards, and Mr. Boulenger considered it incumbent on himself to defend the practice of using such characters;[3] he aptly replied that the use of "purely external characters . . . does not meet the requirements of modern science," and that classifications are not made simply "for the convenience of beginners."

At last, however, the principles of classification adopted by Cope have become generally accepted, and doubtless this was in no small degree hastened by their application to all the amphibians and reptiles by Boulenger.

Cope's attention to the extinct reptiles was excited by the examination and consideration of a Carboniferous lizard-like amphibian which he was requested in 1865 to report upon. It was a new species which he named *Amphibamus grandiceps* and considered to be the type of a new order to which the name Xenorachia was applied, but which he subsequently referred to the new comprehensive order Stegocephali.

[1] Mivart in Proc. Zool. Soc., London, 1869, p. 281.
[2] Cope in Am. Journ. Sci. (3), I, p. 203.
[3] Boulenger in Ann. and Mag. Nat. Hist. (5), XIX, 385.

He sought for specimens of the extinct species with as much enthusiasm as he had for the recent. Extinct and living he considered together and light was mutually reflected from the two to guide him in the perfection of the entire system. In 1869 he gave expression to the results of his studies in a well illustrated "Synopsis of the Extinct Batrachia, Reptilia and Aves of North America." This was supplemented in 1874 by addenda and a "Catalogue of the air-breathing Vertebrata from the coal measures of Ohio."

A rich field was opened to him in 1877, when he received the first instalment of reptilian remains from Texas, which were at first considered to be of Triassic age, but subsequently determined to be Permian. Successive instalments of amphibian as well as reptilian skeletons enriched his collection, and his investigations revealed a new and wonderful fauna rich in species and often differing widely from any previously known. These were described in many articles. The results for the amphibians were summarized in 1884 in a memoir on the "Batrachia of the Permian period of North America."

The Permian amphibians were found to vary much in the composition of their backbones. Instead of having single centra arranged in a continuous row as in existing vertebrates, they had distinct bones on which were devolved portions of the functions fulfilled by the centra of higher vertebrates. Some had "the vertebral bodies represented by three segments each, a basal intercentrum and two lateral pleurocentra;" these were named "Ganocephali" and "Rhachitomi." Some "differ remarkably from all other Vertebrata in having between the centra another set of vertebral bodies, so that each arch has two corresponding bodies;" these were called "Embolomeri."

In tracing the development of these bones, Cope came to the conclusion that they were only partially represented in higher or more specialized types; they did not become consolidated, but one or the other became reduced and finally lost or at least greatly atrophied. In the living amphibians the vertebral centra are homologous only with the intercentra, while, on the contrary, the centra of the reptiles, birds and mammals are represented by the pleurocentra of the Rhachitomes.

The studies of Cope on those classes which had earliest attracted his attention were more nearly completed than for any others. Many years ago he had contemplated the publication of monographs of the amphibians and reptiles of North America and happily he had at last finished his work.

In 1889 his monograph of the "Batrachia of North America" was given to the world as a Bulletin of the United States National Museum (No. 34). It forms a goodly volume of 525 pages illustrated by 81[1] plates and 120 figures inserted in the text. No large country has a more elaborate and scientific exposition of the class than is given in this volume. A synopsis is furnished of all the families and genera wherever found, and detailed descriptions are supplied for all the groups and species represented in the zoölogical realm of North America; 31 genera and 107 species are recognized, and of these Cope had first made known about a quarter, 7 of the genera and 27 of the species having been described by himself.

Shortly before his death, and during his last visit to Washington, he delivered to the National Museum the report on all the reptiles of North America which he had been long preparing. This was prepared on the model of his "Batrachia of North America," but will, of course, be a much larger work, inasmuch as there are nearly three times as many reptiles as batrachians.[2] His last elaborate memoirs dealt with special anatomical features of the serpents and lizards, which he examined with the view of perfecting the system of those groups.

IV.

In 1864[3] Cope became especially interested in the fresh-water fishes of the United States and then, as well as in succeeding years, published enumerations and descriptions of many species. His first papers in 1864 and 1865 were "On a blind Silurid from Pennsylvania" and a "Partial catalogue of the cold-blooded Vertebrata of Michigan;" in 1868 he published "On the distribution of fresh-water fishes in the Allegheny region of southwestern Virginia," and in 1869 appeared a "Synopsis of the Cyprinidæ of Pennsylvania." In addition to these, various minor papers were published and in some of them marine forms were considered.

When in Europe he had purchased a large collection of skeletons of fishes from all parts of the world prepared by Professor Joseph Hyrtl, of Vienna, one of the most skillful practical anato-

[1] The last plate is numbered 86, but five were cancelled, 80, 81, 82, 84 and 85.

[2] Cope's monograph of the reptiles will not include the tortoises, those having been left to Dr. G. Baur to monograph.

[3] A short unentitled communication (before alluded to) was published as early as 1862.

mists of the day. He had a number of other skeletons made to represent missing types. With these as a basis he proceeded to recast the classification of fishes. The first contribution to the subject was embodied in an introductory chapter of his "Contribution to the Ichthyology of the Lesser Antilles," published early in 1871.

The same chapter, with the same title, "Observations on the Systematic Relations of Fishes," but with some modifications and additions, was later published in the Proceedings of the American Association for the Advancement of Science for 1871. This was a notable paper and replete with original observations of value. It was not, however, up to the standard of his work on amphibians and reptiles. The subject, indeed, was too vast and only a superficial examination was made of special parts. It was not a classification based on the examination of the entire structure, but rather an exposition of the development of a few particular characters, which more experience subsequently convinced him were of less value than he had supposed. Nevertheless, in some respects the proposed classification was much in advance of those previously adopted, and useful hints were given for the further improvement of the system.

Later Cope followed up this attempt at the reformation of the ichthyological system with several others especially treating of extinct types. One of them, "On the classification of extinct fishes of the lower types," was published in the Proceedings of the American Association for 1877. The results of his studies were summarized, in 1889, in "A synopsis of the families of Vertebrata," and two years afterwards (1891) with modifications, in an article "On the non-actinopterygian Teleostomi." These results were very valuable and attention was for the first time directed to the importance and morphological significance of the skeletal fin structures of the ancient fishes long confounded under the name of Ganoids. Instead of this single order (or sub-class) of the old systematists, he named four superorders of the Teleostomi or "true fishes," and recognized seven orders, including the old ganoids after eliminating the Lepidosteids and Amiids, which were referred to the Actinopterygians. Only two of the seven orders are represented by existing forms — one (*Cladistia*) by the bichirs of Africa and the other (*Chondrostei*) by the sturgeons.

His work on the extinct fishes was incomparably better than any

that had been done oefore in the United States. He far surpassed all his predecessors, not only by his knowledge of morphological details manifest in the extinct as well as in living forms, but by his keen philosophical instinct and taxonomic tact. But this philosophical instinct was sometimes at fault, and occasionally he indulged in the wildest speculations, for which he has, not unjustly, been taken to task. Yet even his blunders were the result of the facility of his mind in seizing and adapting the latest utterances of science. One notorious case may be given. The great Russian embryologist Kowalevsky published a memoir sustaining the thesis that the Tunicates were members of the vertebrate phylum and that the larval stage of most of the species had the homological equivalent of the backbone of the true vertebrates. Cope foresaw the morphological consequences of this view and sought the vertebrates nearest the Tunicates. He settled upon some strange forms of the Silurian and Devonian times known as Pteraspids and Cephalaspids. They were the earliest known of vertebrates and therefore likely to be the most primitive in structure. Most of them had a shell-like encasement, composed of bone-like plates. He happened to find illustrations of the living *Chelyosoma*, a true Tunicate having a system of plate-like indurations of the integument, somewhat similar in appearance to those of some of the ancient fishes. It was assumed that this mere superficial similarity indicated genetic relationship. To those acquainted with the structure of *Chelyosoma* this approximation seemed strange indeed; its anatomy was known and the form is simply a well marked relation of the typical ascidiids, but highly specialized by development of integumentary plate-like horny indurations. Histologically and otherwise they were very different from the plates of the extinct armored vertebrates. Cope's guess was simply the result of the tendency to jump at conclusions which he was constantly obliged to curb, and unfortunately he rushed into print before he had time to think. He soon reconsidered the case with calmer mind, and abandoned his hypothesis. Few men were ever more willing to reconsider evidence and retrace false steps than was he.

In spite of errors of detail and somewhat hasty generalization, the ichthyological labors of Cope were unusually valuable contributions to science, and the progress of ichthyology has been much accelerated, not only by these labors, but by the investigations they challenged.

V.

Cope's attention was early drawn to the mammals. His first published article (1863) was a description of a supposed new shrew found in New Hampshire, and in 1865 he described various cetaceans. In 1868 he began the collection and investigation of the fossil mammals of the western territory, and thenceforward devoted the larger share of his attention to the description and restoration of the numerous new species which he from time to time brought to light. The previous investigators of the extinct mammals of America had almost exclusively confined themselves to descriptions and illustrations of the crania and dentition, but a new era was introduced when Marsh and Cope sent out exploring expeditions or themselves collected. No parts of skeleton were neglected; all were collected. Gradually the numerous bones from different parts of the skeleton were identified, and finally many of the beasts of old were resurrected into skeletons almost as complete as those just divested of muscles.

The discoveries resulting from such thorough work quite modified or even overturned old conceptions. It became evident that there was a great contrast between the development of the mammals and that of invertebrates, and even, though in a less degree, of fishes. It appeared that there was a much more rapid process of evolution for the mammals than for the lower classes. All the mammals of the oldest of the Tertiary periods were strange and very unlike those of recent times, and no descendants of even the same families lived to be the contemporaries of civilized man. The views of the founder of vertebrate paleontology were also to a considerable extent subverted. Cuvier taught that there was always a coördination between the various systems of the animal frame and that, from the remains or impress of one part, the approximate structure of the other parts could be inferred. He even pushed this doctrine to such an extreme that he overlooked some obvious counter-facts. One such case is so remarkable because it originated with Cuvier and was endorsed by Huxley[1] that it is worthy of mention here, and Huxley's introduction to it and translation of it may be given. Huxley himself protests against the too literal application of Cuvier's law and recalls Cuvier's own reserve:

[1] Huxley, "Introduction to the Classification of Animals," 1869, in first chapter "On Classification in General."

"Cuvier, the more servile of whose imitators are fond of citing his mistaken doctrines as to the nature of the methods of paleontology against the conclusions of logic and of common sense, has put this so strongly that I cannot refrain from quoting his words:[1]

"But I doubt if any one would have divined, if untaught by observation, that all ruminants have the foot cleft, and that they alone have it. I doubt if any one would have divined that there are frontal horns only in this class; that those among them which have sharp canines for the most part lack horns.

"However, since these relations are constant, they must have some sufficient cause; but, since we are ignorant of it, we must make good the defect of the theory by means of observation. It enables us to establish empirical laws, which become almost as certain as rational laws, when they rest on sufficiently repeated observations; so that now, whoso sees merely the print of a cleft foot may conclude that the animal which left this impression ruminated, and this conclusion is as certain as any other in physics or morals. This footprint alone, then, yields to him who observes it, the form of the teeth, the form of the jaws, the form of the vertebræ, the form of all the bones of the legs, of the thighs, of the shoulders, and of the pelvis of the animal which has passed by. It is a surer mark than all those of Zadig."

The first perusal of these remarks would occasion surprise to some and immediately induce a second, more careful, reading to ascertain whether they had not been misunderstood. Some men, with much less knowledge than either Cuvier or Huxley, may at once recall living exceptions to the positive statements as to the coördination of the "foot cleft" with the other characters specified. One of the most common of domesticated animals — the hog — would come up before the "mind's eye," if not the actual eye at the moment, to refute any such correlation as was claimed. Nevertheless, notwithstanding the fierce controversial literature centered on Huxley, no allusion appears to have been made to the lapsus. Yet every one will admit that the hog has the "foot cleft" as much as any ruminant, but the "form of the teeth" and the form of some vertebræ are quite different from those of the ruminants, and of course the multiple stomach and adaptation for rumination do not exist in the hog. That any one mammalogist should make such a slip is not very surprising, but that a second equally learned should follow in his steps is a singular psychological curiosity.

I need scarcely add that the law of correlation applied by Cuvier to the structures of ruminants entirely fails in the case of many

[1] Ossemens fossiles, ed. 4^{me}, tome 1^{r}, p. 184.

extinct mammals discovered since Cuvier's days. Zadig would have been completely nonplussed if he could have seen the imprint of an Agriochœrid, a Uintatherid or a Menodontid.

I have given this quotation for two reasons: first, to indicate how the increase of our knowledge has revolutionized old conceptions; and, second, to show how even the ablest of men may stumble.

Cope has been much criticised for the mistakes and false generalizations he made. Unquestionably he did make many. But error seems to be inseparable from investigation, and if he made more than the other great masters he covered more ground and did more work. He was also, it must be admitted, more hasty than some others in that he availed himself of the more frequent means of publication he enjoyed.

The great merit of Cope's work on mammals is that he always considered the old and new — the extinct and recent — forms together. He refused to be bound by consistency or by precedent, either set by himself or others. Fresh discoveries opened new vistas to him, and he modified his views from time to time and as often as he received new evidence.

He introduced many new families in the system and sought to improve the system by the comparison of all the elements of the skeleton. He came to the conclusion that the affinities of the ungulate quadrupeds were best expressed by the manner of articulation of the bones of the carpus and tarsus; he associated those having the "carpal and usually tarsal bones in linear series" in a great order which he called Taxeopoda, and contrasted them with the Proboscidea and typical Ungulata, which he named anew Diplarthra. In the Taxeopoda he gathered many extinct families and associated with them forms of the existing fauna known as the Hyracoidea, Daubentonioidea, Quadrumana and Anthropomorpha. I cannot altogether assent to this collocation inasmuch as I think the common characteristics of the three groups last mentioned — especially the structure of the brain and the development of the posterior cornua of the ventricles as well as calcarine sulci — justify the old order Primates. Nevertheless an important character was first appreciated in the composition of the podial bones, and fresh insight was obtained into the relations of ancient types.

I can only name a few more of Cope's discoveries in this connection. One was the generalization of "trituberculy," or the original

development of three tubercles to molar teeth, and that subsequent modifications of the corresponding teeth were based on this original plan. Another was the remarkable *Phenacodus* of the Eocene, which was considered to be nearly in the line of descent for the ungulates as well as the series culminating in man and which led him to the conception of the taxeopodous group.

The past history and genealogy of the camels and their relations were likewise elucidated. In the present epoch only two nearly related types exist separated by half the globe — the true camels of central and northern Asia and the llamas of the Peruvian Andes. Cope revealed numerous species from various Tertiary beds and showed that the type was originally richly developed in America.[1]

VI.

Paleontology from more than one point of view may be divided into Invertebrate and Vertebrate. The subjects of the former are generally to be found in an approximately complete condition so far as the exterior is concerned, and early attracted the attention of investigators, often little familiar with recent zoölogy, and received names. The subjects of the latter — especially the higher types, as mammals, birds and reptiles — are rarely found, except in a fragmentary condition. Special knowledge of osteology, even to its minutest details, is requisite to deal successfully with such remains. Consequently the fossil vertebrates of the United States were neglected and left to the few who had cultivated the requisite knowledge to deal with them.

Another reason existed for the tardy attention to vertebrate paleontology, which continued till nearly the last quarter of our present century in the United States. No deposits containing many fossil vertebrate remains had become known in the East. Zoölogists interested in the past and in the genealogy of existing forms lamented the poverty of the United States, which contrasted with the richness of some parts of Europe. It was even thought that there was no hope of finding here such trophies of the past as the beds of the Paris Basin or those of Grecian Pikermi had yielded to European paleontologists. But all this was to be changed.

[1] Professor Osborn, in a recent letter, has justly remarked that "in the Mammals I hardly feel you do Cope sufficient justice, his work has been so potent." The exigencies of time and space alone prevented me from doing that justice and I may remedy the defect later.

Rumor had long before hinted that numerous skeletal remains could be found in certain parts of the wild West, but the information was very vague. Enough was known, however, to induce Professor Marsh to visit certain deposits of which he had heard. In 1870 he explored an Eocene lake-basin in Wyoming, drained by the Green river, the main tributary of the Colorado, and therein found numerous bones, belonging to almost all parts of the skeleton, of some remarkable gigantic mammals which he called Dinocerata. The results of this exploration interested Cope in the highest degree. He visited the same region in 1872, and thenceforth his attention to the vertebrate paleontology of the Western States and Territories was never interrupted. An intense rivalry arose between Professor Marsh and himself which, in time, it must be confessed became very bitter. Nevertheless, as in most quarrels respecting facts, investigations were provoked by mutual recriminations which resulted in a more speedy accumulation of data and a more critical examination of those data than would have been likely under less perturbed conditions. Most of those data relate to morphological and anatomical considerations, and therefore belong rather to mammalogy and herpetology than to geology.

The relations of the ancient forms to each other in point of time; to those of other lands, and to those whose remains were embedded in other rocks, had necessarily to be investigated. The earliest conclusions of Cope were brought together and published in 1879 in a memoir on "The Relations of the Horizons of Extinct Vertebrata of Europe and North America.[1] He attempted therein to synchronize, or, rather, homotaxially correlate the various ancient faunas of North America and "West Europe" from the "Primordial" to the "Pliocene." Naturally the greater part of the memoir was devoted to the consideration of the Tertiary divisions; of these he admitted for the American faunas six primary divisions, and four of these were dichotomously subdivided. Of the primary divisions three were referred to the Eocene, one (White River) to the Oligocene, one (Loup Fork) to the Miocene, and one to the Pliocene. The exposition thus made represents views not very different from those now held, although, of course, modifications in details have since been necessary.

The evolution of the various animal, and especially mammalian types was also continually the subject of Cope's researches, and

[1] Bull. U. S. Survey Terr., v, 33-54.

he attempted to trace the passage from those of the most ancient periods to those of later ones.[1]

VII.

Cope was not satisfied with the study of morphological details or simple taxonomy. He aspired to know how animals came into existence; why they varied as they did, and what laws determined their being. His was an eminently philosophical mind, but at the same time with a decided tendency to metaphysical speculation. In one of his earliest papers he manifested this tendency and it persisted through life. It is with much hesitation that I venture to give an exposition of his most salient views, for I must confess I do not altogether like his philosophy and am able to subscribe to it only in part. I cannot but wish that one of his numerous disciples could have been chosen for this task. But I must not pass it by, for it is the most characteristic feature of Cope's work and the one he most esteemed.

Cope began his public scientific career, it will be remembered, in the same year in which Darwin's long studies had fructified into his "Origin of Species."

As was quite natural with his keen instincts, Cope early adopted the doctrine of transmutation of species and recognized the truth that all the animals of the present epoch are descendants from those of past times with modifications which separate them as species, and eventually as representatives of genera, of families and orders differing from the earlier ones as we retrace the steps of Time farther and farther back. He was not, however, satisfied with Darwin's theory, and denied that natural selection was a sufficient factor for differentiation. He would not admit that animals were passive subjects and that the slight variations which were manifest in the progeny of species were sufficient to enable Nature to select from and to fit for future conditions. He contended that the volition and endeavors of an animal had much to do with future progeny as well as its own brief life. In short, he claimed that characters acquired by animals through their own efforts, or

[1] I have been reminded by Professor Osborn of Cope's "discovery of the Puerco —Cope's greatest geological achievement which Professor Marsh still fails to recognize; also the definition of the John Day and Deep river beds." Professor Osborn adds that "practically the whole fauna of the Wasatch is also Cope's." I recognized these facts but, as in herpetology and ichthyology, was obliged to limit my address and to refrain from going into details.

forced on them by various external agencies or accidents, might be transmitted to their offspring. He further, first in a chapter in his "Synopsis of the Cyprinidæ of Pennsylvania," outlined and, later, in "The Origin of Genera," he elaborated, a peculiar theory characterized mainly by what he called (with Professor Hyatt) "the law of *acceleration* and *retardation*" in development. Darwin complained that he could never understand this law, and Cope complained that Darwin had not stated his views correctly in an attempted abstract. I therefore give Cope's views, restated in his own language, summarizing them years afterwards. "The following doctrines," he says, "were taught:"

"First, that the development of new characters has been accomplished by an *acceleration* or *retardation* in the growth of the parts changed. This was demonstrated by reference to a class of facts, some of which were new, which gave ground for the establishment of the new doctrine.

Second, that of *exact parallelism* between the adult of one individual or set of individuals and a transitional stage of one or more other individuals. This doctrine is distinct from that of exact parallelism which had already been stated by von Baer. And that this law expresses the origin of genera and higher groups, because,

Third, they can only be distinguished *by single characters* when all their representatives come to be known.

Fourth, that genera and various other groups have descended, not from a single generalized genus, etc., of the same group, but from corresponding genera of one or more other groups. This was called the doctrine of *homologous groups*.

Fifth, the doctrine that these homologous groups belong to different geological periods, and,

Sixth, to different geographical areas, which, therefore, in some instances, are,

Seventh, related to each other in a successional way like the epochs of geological time.

"Of these doctrines it may be observed that the first and second are now the common property of evolutionists, and are recognized everywhere as matter of fact. The names which I selected to express them have, however, only come into partial use. The author believes that, although the doctrine was vaguely shadowed out in the minds of students prior to the publication of this essay, it had not previously been clearly expressed, nor been reduced to a demonstration. Of the truth of the doctrine the author is more than ever convinced, and he believes that paleontological discovery has demonstrated it in many instances, and that other demonstrations will follow. The fourth proposition (that of homologous groups) is now held as a hypothesis explaining the phylogeny of various groups of animals. For the descent of one homologous group from another, the term *polyphyletic* has been coined. It remains to be seen whether the doctrine is of

universal application or not. That homologous groups belong to different geological horizons, as stated under the fifth head, has been frequently demonstrated since the publication of the essay. That the sixth proposition is true in a certain number of cases is well known, and it follows that the seventh proposition is also true in those cases. The latter hypothesis, which was originally advanced by Professor Agassiz, is, however, only partially true, and the advance of paleontological study has not demonstrated that it has had a very wide application in geological time.

"A proposition which was made prominent in this essay was that the prevalence of non-adaptive characters in animals proves the inedequacy of hypotheses which ascribe the survival of types to their superior adaptation to their environment. Numerous facts of this kind undoubtedly indicate little or no activity of a selective agency in nature, and do point to the existence of an especial developmental force acting by a direct influence on growth. The action of this force is the acceleration and retardation appealed to in this paper. The force itself was not distinguished until the publication of the essay entitled 'The Method of Creation' [1871], where it was named growth-force, or bathmism. The energetic action of this force accounts for the origin of characters, whether adaptive or non-adaptive, the former differing from the latter in an intelligent direction, which adapts them to the environment. The numerous adaptive characters of animals had by that time engaged the attention of the author, and he found that they are even more numerous than the non-adaptive. Some of the latter were accounted for on the theory of the 'complementary location of growth-force.'"

We can only consider the "law of acceleration and retardation." Again it behooves us to seek his own definition:

"*a.* The succession of construction of parts of a complex was originally a succession of identical repetitions; and grade influence merely determined the number and location of such repetitions.

"*b.* *Acceleration* signifies addition to the number and location of such repetitions during the period preceding maturity, as compared with the preceding generation, and *retardation* signifies a reduction of the number of such repetitions during the same time."[1]

His meaning may best be inferred from his application to mankind. This was done in the following terms in 1872:[2]

"Let an application be made to the origin of the human species. It is scarcely necessary to point out at the start the fact, universally admitted by anatomists, that man and monkeys belong to the same order of Mammalia, and differ in those minor characters, generally used to define a 'family' in zoölogy.

[1] Proc. Am. Phil. Soc., 1871; Origin of the Fittest, p. 182.
[2] Penn. Monthly Mag. 1872; Origin of the Fittest, p. 11, 1887.

"Now, these differences are as follows: In man we have the large head with prominent forehead and short jaws; short canine teeth without interruption behind (above); short arms, and thumb of hand not opposable. In monkeys we have the reverse of all these characters. But what do we see in young monkeys? A head and brain as large relatively as in many men, with jaws not more prominent than in some races; the arms not longer than in the long-armed races of men, that is, a little beyond half way along the femur. . . . At this age of the individual the distinctive characters are those of *homo*, with the exception of the opposable thumb of the hind foot, and the longer canine tooth. . . .

"Now in the light of various cases observed, where members of the same species or brood are found at adult age to differ in the number of immature characters they possess, we may conclude that man originated in the following way: that is, by a delay or retardation of growth of the body and fore limbs as compared with the head; retardation of the jaws as compared with the brain case, and retardation in the protrusion of the canine teeth."

There is good reason for thinking that fallacy is involved in this argument and that quite a different interpretation should be put on the evolution of the characters in question. It is not the fore limbs that are retarded in man, but the hind limbs have become enlarged (compare the adult and the infant). There is not retardation of the jaws, but a special teleological adaptation. Man has for the most part at least discontinued the use of his teeth for warfare, and as a result of diminished use the canines have been reduced and the diastemata of the dental series obliterated. The brain has grown after birth and become enlarged and, as a consequence, the brain case has extended forward — the reverse of what occurs in the apes. Concomitantly with the diminished use of the teeth and jaws, the masseter and temporal muscles have become reduced, and the sagittal and lambdoidal ridges have consequently become atrophied. The ecarinate rounded voluminous calvarium is the result.

It has been claimed that the young of higher species "are constantly accelerating their development." In man, however, development is retarded, inasmuch as infancy and juvenility are prolonged far beyond the periods observed in our simian relatives.

Such examples as this give cause to believe that the "law of acceleration and retardation" has been at least unduly extended. Acceleration and retardation are, however, to a large extent, terms which express facts of evolution; whether the word law is applicable may depend on the meaning one gives the word.

The transmission of acquired characters was one of the accepted and most cherished dogmas of Cope, and the belief in transmissibility of such characters is an essential of the creed of so many who have become his followers in America, that a special school came into existence known as the Neo-Lamarckian and also as the American School. My own prejudices have inclined me to that school. Nevertheless, when I have divested myself of such prejudices as well as I could, I have been compelled to admit that the evidence of the heredity of acquired characters was rather weak. There was, indeed, evidence for, as well as against, but that against the doctrine of the transmissibility of acquired characters seems to be the more weighty.

It is to be understood that the acquired characters considered in this connection are such as have been developed during postnatal life as a result of endeavors of the animal or of the influence of external agencies. The evidence presented has been mostly in support of the contention that the characters acquired have been directly inherited by offspring, and consequently the transition from the form not possessing the character to one having it is rapid. The evidence adduced has not been conclusive, to say the least. There is, apparently, a germ of truth in the proposition that acquired characters are transmitted, but in a modified sense, and the case has been weakened rather than strengthened by the evidence afforded.

The evidence for inheritance of acquired characters was frequently given by Cope and in his last published work — "The Primary Factors of Organic Evolution," — he marshalled the testimonies of many witnesses with his accustomed skill. He evoked "evidence from embryology," "evidence from paleontology," "evidence from breeding;" he considered the "characters due to nutrition," "characters due to exercise of function," "characters due to disease," "characters due to mutilation and injuries," and "characters due to regional influence;" he inquired into "the conditions of inheritance," and he fought against the "objections to the doctrine of inheritance of acquired characters." I have gone over all this evidence and yet I have not been convinced that the contention has been sustained that characters acquired during the external life of an animal are transmitted. Many cases are alleged to sustain the "inheritance of characters due to mutilation and injuries." Some of these may be considered as mere coincidences; others provoke

skepticism for one reason or another. To discuss them would be out of place here. But at least we may meet evidence with counter-evidence.

On the one hand, all the data and experiments recapitulated in the cases enumerated concern only two, or at most very few, generations of the animals in question, and were within the compass of a single man's lifetime.

On the other hand, we have data and observations of the most reliable nature, and of an extraordinary compass. These have resulted not from experiments for the determination of a specific question, but from observances of a religious character. They were really in the nature of surgical operations, but for our purpose may be looked upon as experiments and have the value of contrived experiments. In no other field has such a series of disinterested experiments been available. They were conducted on countless millions of mankind and for thousands of years. The subjects experimented upon were kept isolated from others alike by their own prejudices and the prejudices of their neighbors. Circumcision is the term applied to the experiments in question.

For about four thousand years circumcision has been practised on a gigantic scale. Every male child among the Jews was operated upon, not only in Palestine, but wherever representatives of the race had wandered and adhered to their religion; religion itself was involved in the operation and it was regarded as a holy rite; the most scrupulous attention was paid to details. The operation was performed eight days after birth, and consequently there could be no functional activity of the tissues concerned. But after four thousand years the new-born boys of the race come into the world with the special integument developed as much as in those of other races. Even the principle of atrophy through disuse has not become manifest in the case.

Other evidence, it seems to me, is the result of confounding the potentiality of a function with its manifestation. I allude to one set of examples on account of the interest of the cases, and I do so with the deference due to the eminence and ability of the gentleman who has furnished the evidence. That evidence has been collected under the head of "inheritance of characters due to the exercise of function." The evolution of the American trotting horse was considered. It was recorded that "by 1810 the taste for trotting as a sport had . . . increased here, and in 1818

it became a recognized sport under specific rules." . . . "At the end of 1824, six years after the first accepted three-minute record, the record had fallen to 2:34." . . . "By 1848 the record was lowered to 2:29½; the next decade lowered the record five seconds." Finally, at the close of 1895, the record had been further lowered to 2:03¾. . . . It is deduced from these premises that "there is nothing whatever in the actual phenomena observed anywhere along the line of this development of speed that would lead us to suspect even that the changes due to exercise of function had *not* been a factor in the evolution." But to me it seems that there is no evidence to show that the speed attained was other than would have resulted from taking the same animals untrained and then speeding the last. The speed is, of course, simply the expression of functional adaptation, and the horses were selected merely because, by their manifestation, they showed that they had the coördination of structural and psychological characters needed for the manifestation of the function. The manifestation guided the breeder to the selection of the animals. The successful animals were the pick of thousands unknown to fame.

But there is much in the history of the development of animals that seems to lead to the belief that eventually modifications may be due in part to acts of representatives of the phylum to which they belong. It is difficult to believe that some structural features are simply the result of natural selection operating on chance variations An application of the doctrine of chances to some such cases appears to be adverse to the conception that they represent the influence of natural selection unaided.

A feature characteristic of most cave animals of widely diverse groups and classes is the atrophy of the eyes, and it seems to be most logical to attribute this to disuse of those organs in remote progenitors, and to assume that the atrophy may have resulted from a failure of nourishment by the nutrient fluid of the organs on account of the loss of functional activity rather than to selection by nature of forms with successively diminishing eyes. The presence of eyes in most cases certainly would scarcely be an element of disadvantage to animals, and it may be allowable to invoke some other agency than chance selection. We may be justified in postulating that the continuous disuse of the organs would in time react on the nutrition of the parts affected, and finally atrophy or disappearance

would result. Like explanation would be applicable to the innumerable cases of atrophy of parts known to the naturalist.

But if cessation of nutrition culminates in final atrophy, increased nutrition of parts may result in hypertrophy and increased nutrition may be the concomitant of increased activity of parts. The exercise of such parts continued for many generations may react on the organization and the progeny at length be affected thereby. Of such cases Cope adduced many examples. The feet of the horse line furnish illustrations. The existing horse has the median toes and hoofs greatly hypertrophied and the lateral ones atrophied, but the remote ancestors had feet of nearly the same general pattern as the rhinoceroses and tapirs. Atrophy of the lateral digits has progressed inversely to hypertrophy of the middle ones. An analogous line of development culminating in feet superficially much like those of the horse was followed by another quite remote family of hoofed mammals, the Prototheriids of South America.

The idea of acceleration and retardation was associated by Cope with the idea that the course of evolution was determined from the beginning of things, and that life, to use his own words, is "*energy directed by sensibility or by a mechanism which has originated under the direction of sensibility.*" He maintained that "consciousness as well as life preceded organism," and he called this conception "the hypothesis of archæsthetism." This idea I refer to especially because it was broached in his vice-presidential address, delivered at the meeting of the American Association for the Advancement of Science in Philadelphia in 1884.[1]

I am myself unable to comprehend consciousness except as a product or result of organization, and those who wish to learn more about Cope's views respecting the question must refer to one of his many papers.

Whatever may be thought of Cope's philosophical views, his presentation of them is always interesting and some of them are illustrated with a wealth of facts that renders his communications valuable as repertories of well digested information. His first special paper, on "The Origin of Genera," published as early as 1868, is especially noteworthy for the mass of morphological data contained in it and for the apt manner in which they are tabulated.

[1] Origin of Fittest, p. 425.

VIII.

I venture to conclude with reflections on the rank that may be assigned to Cope in the world of science.

Among those that have cultivated the same branches of science that he did — the study of the recent as well as the extinct vertebrates — three naturalists have acquired unusual celebrity. Those are Cuvier, Owen and Huxley.

Cuvier excelled all of his time in the extent of his knowledge of the anatomical structure of animals and appreciation of morphological details, and first systematically applied them to and combined them with the remains of extinct vertebrates, especially the mammals and reptiles. He was the real founder of Vertebrate Paleontology.

Owen, a disciple of Cuvier, followed in his footsteps, and, with not unequal skill in reconstruction and with command of ampler materials, built largely on the structure that Cuvier had begun.

Huxley covered as wide a field as Cuvier and Owen, and likewise combined knowledge of the details of structure of the recent forms with acquaintance with the ancient ones. His actual investigations were, however, less in amount than those of either of his predecessors. He excelled in logical and forcible presentation of facts.

Cope covered a field as extensive as any of the three. His knowledge of structural details of all the classes of Vertebrates was probably more symmetrical than that of any of those with whom he is compared; his command of material was greater than that of any of the others; his industry was equal to Owen's: in the clearness of his conceptions he was equalled by Huxley alone; in the skill with which he weighed discovered facts, in the aptness of his presentation of those facts, and in the lucid methods by which the labor of the student was saved and the conception of the numerous propositions facilitated, he was unequalled. His logical ability may have been less than that of Huxley and possibly of Cuvier. He has been much blamed on account of the constant changes of his views and because he was inconsistent. Unquestionably he did change his views very often. Doubtless some of those changes were necessitated by too great haste in formulation and too great rashness in publication. The freedom to change which he exercised, and which was exercised too little by at least one of his predeces-

sors, was an offset to his rashness. He exercised a proper scientific spirit in refusing to be always consistent at the expense of truth.

His reputation at present is much inferior, at least among the people at large, to those of the men with whom he has been compared. Immediate reputation depends on various circumstances, some of which are quite adventitious, and it is often long before men find their true levels. It is scarcely premature to prophesy that Cope's reputation will grow and that in the future history of science his place will be at least as large as that of any of his predecessors.

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ADDRESS

BY

W. W. BEMAN,

VICE PRESIDENT, AND CHAIRMAN OF SECTION A.

ERRATA.

On page 34, 4th line from top of page, for 811-44 read 81-144.

On page 44, 6th line from bottom of page, for 1779 read 1799.

This paper, which deals with the geometric representation of imaginary quantities, which was read and printed several years before the famous essay of Argand and contains fully as exact a treatment of the subject, lay buried for nearly a century until attention was again drawn to it in 1895 by a thesis of S. D. Christensen upon the development of mathematics in Denmark and Norway in the eighteenth century.

Inasmuch as this memoir of Wessel's is still comparatively unknown, I have thought that it would not be uninteresting at this time to present a sketch of the development of the geometric treatment of the imaginary, particularly in the latter part of the eighteenth century and the first part of the nineteenth.

We find the square root of a negative quantity appearing for the first time in the *Stereometria* of Heron of Alexandria, 100 B. C. After having given a correct formula for the determination of the volume of a frustum of a pyramid with square base and applied it

successfully to the case where the side of the lower base is 10, of the upper 2, and the edge 9, the author endeavors to solve the problem when the side of the lower base is 28, of the upper 4, and the edge 15. Instead of the square root of 81–144 required by the formula, he takes the square root of 144–81 and calls it equal to 8 less $\frac{1}{8}$, *i. e.*, he replaces $\sqrt{-1}$ by 1, and fails to observe that the problem as stated is impossible. Whether this mistake was due to Heron or to the ignorance of some copyist cannot be determined.

In the solution of the problem to find a right angled triangle whose perimeter is 12 and area 7, Diophantus, in his *Arithmetica*, 300 A. D., reaches the equation $336x^2 + 24 = 172x$ and says that the equation cannot be solved unless the square of the half coëfficient of x diminished by the product of 24 and the coëfficient of x^2 is a square. No notice is taken of the fact that the value of x in this equation actually involves the square root of a negative quantity.

Bhaskara, born 1114 A. D., in his chapter *Vija Ganita*, was able to go a step further. He gave the rule:

> The square of a positive number as also of a negative number is positive and the square root of a positive number is two-fold, positive and negative. There is no square root of a negative number, for this is not a square.

The first mathematician who had the courage actually to use the square root of a negative number in computation was Cardano. At an earlier period he had declared such a quantity to be wholly impossible, but in the *Ars Magna*, 1545, he discusses the problem of dividing 10 into two parts whose product shall be 40 and obtains the values $5+\sqrt{-15}$, $5-\sqrt{-15}$. These he verifies by multiplication. Such quantities he calls sophistic, since it is not permissible to operate with them as with pure negative numbers or others, nor to assign them a meaning.

Bombelli, in his *Algebra*, 1572, gives a number of rules for the use of such quantities as $a+b\sqrt{-1}$, but makes no endeavor to explain their character.

Girard knew that every equation has as many roots as its degree indicates and consequently recognized the existence of imaginary roots. In his *Invention nouvelle en l'algèbre*, 1629, while discussing the roots of the equation $x^4-4x+3=0$ he asks what purpose is subserved by such roots as $-1+\sqrt{-2}$ and $-1-\sqrt{-2}$ and says that they show the generality of the law of formation of the coëfficients and are useful of themselves.

Descartes, in his *Geometria*, 1637, gives us no new ideas upon the subject, but is the first to apply the terms real and imaginary by way of contrast to the roots of an equation.

Wallis, in his *Treatise of Algebra*, 1685, leads the van in his endeavor to give a geometric interpretation to the square root of a negative number. In chapter LXVI we read:

These *Imaginary* Quantities (as they are commonly called), arising from the *Supposed* Root of a Negative Square (when they happen,) are reputed to imply that the Case proposed is Impossible.

And so indeed it is, as to the first and strict notion of what is proposed. For it is not possible that any Number (Negative or Affirmative) Multiplied into itself can produce (for instance) -4. Since that Like Signs (whether $+$ or $-$) will produce $+$; and therefore not -4.

But it is also Impossible that any Quantity (though not a Supposed Square) can be *Negative*. Since that it is not possible that any *Magnitude* can be *Less than Nothing* or any *Number Fewer than None*.

Yet is not that Supposition (of Negative Quantities,) either Unuseful or Absurd; when rightly understood. And though, as to the bare Algebraick Notation, it import a Quantity less than nothing: Yet, when it comes to a Physical Application, it denotes as Real a Quantity as if the Sign were $+$; but to be interpreted in a contrary sense.

He illustrates this by distances measured forward and backward upon a straight line in the usual way, and continues:

Now what is admitted in Lines must, on the same Reason, be allowed in Plains also.

Having thus justified the existence of negative planes, he goes on:

But now (supposing this Negative Plain, -1600 Perches, to be in the form of a Square;) must not this Supposed Square be supposed to have a Side? And if so, what shall this Side be?

We cannot say it is 40, nor that it is -40 * *

But thus rather that it is $\sqrt{-1600}$, or * * $10\sqrt{-16}$, or $20\sqrt{-4}$, or $40\sqrt{-1}$.

Where $\sqrt{\ }$ implies a Mean Proportional between a Positive and a Negative Quantity. For like as $\sqrt{bc}$ signifies a Mean Proportional between $+b$ and $+c$; or between $-b$ and $-c$; ** So doth $\sqrt{-bc}$ signify a Mean Proportional between $+b$ and $-c$, or between $-b$ and $+c$.

In chapter LXVII Wallis gives a geometric exemplification of a mean proportional, interpreting $\sqrt{bc}$ as a sine in a circle whose diameter $= b + c$, and $\sqrt{-bc}$ as a tangent in a circle whose diameter $= -b + c$. He then finds the base of a triangle when the two sides and the angle opposite, and hence the altitude, are given.

Assuming $AP = 20$, $PB = 15$, and the altitude $PC = 12$, by the use of the triangle BCP, right-angled at C, he obtains two real values for the base AB. Then taking $AP = 20$, $PB = 12$, and the altitude $PC = 15$, he finds imaginary values for the base.

These he interprets by saying:

This Impossibility in *Algebra* argues an Impossibility of the case proposed in Geometry; and that the point B cannot be had (as supposed,) in the Line AC, however produced (forward or backward,) from A.

Yet are there Two Points designed (out of that Line, but) in the same Plain; to either of which, if we draw the Lines AB, BP, we have a Triangle; whose Sides AP, PB, are such as were required: And the Angle PAC, and Altitude PC, (above AC, though not above AB,) such as was proposed:

In this case he takes the triangle BCP to be right angled at B. Further:

And (in the Figure,) though not the Two Lines themselves, AB, AB, (as in the First case, where they lay in the Line AC;) yet the Ground-Lines on which they stand, $A\beta$, $A\beta$, are equal to the Double of AC: That is, if to either of those AB, we join Ba, equal to the other of them, and with the same Declivity; ACa (the distance of Aa) will be a Streight Line equal to the double of AC; as is ACa in the First case.

The greatest difference is this; that in the first Case, the Points B, B, lying in the Line AC, the Lines AB, AB, are the same with their Ground-Lines, but not so in this last case where B, B are so raised above $\beta\beta$ (the respective Points in their Ground-Lines, over which they stand), as to make the case feasible; (that is, so much as is the versed Sine of CB to the Diameter PC:) But in both ACa (the Ground-Line of ABa) is equal to the Double of AC.

So that, whereas in case of Negative Roots, we are to say, The Point B cannot be found, so as is supposed in AC Forward, but Backward from A it may in the same Line: We must here say, in case of a Negative Square, the Point B cannot be found so as was supposed, in the Line AC; but Above that Line it may in the same Plain. This I have the more largely insisted upon, because the Notion (I think) is new; and this, the plainest Declaration that at present I can think of, to explicate what we commonly call the *Imaginary Roots* of Quadratic Equations. For such are these.

From these extracts it is evident that Wallis possessed, at least in germ, some elements of the modern methods of addition and subtraction of directed lines.

For the next hundred years no advance of importance was made. Euler, for example, makes large use of the imaginary, but in his *Algebra*, 1770, he observes:

All such expressions as $\sqrt{-1}$, $\sqrt{-2}$, etc., are consequently impossible or imaginary numbers, since they represent roots of negative quantities: and of such numbers we may truly assert that they are neither nothing, nor greater than nothing, nor less than nothing, which necessarily constitutes them imaginary or impossible.

On the 10th of March, 1797, a surveyor named Wessel presented to the Royal Academy of Sciences and Letters of Denmark a memoir "On the Analytic Representation of Direction," which was printed in 1798 and appeared in Vol. V of the Memoirs of the Academy in 1799.

Caspar Wessel was born June 8, 1745, at Jonsrud, in Norway, where his father was a pastor. Though one of thirteen children, he had a good education, for in 1757 he entered the high school at Christiania and in 1763 went to Copenhagen to pursue further studies. In 1764 he was engaged by the Academy of Sciences as an assistant in the triangulation and preparation of a map of Denmark. Till 1805 he remained in the continuous employ of the Academy as surveyor. Wessel was highly esteemed by his contemporaries, and for some special work done after leaving the service of the Academy he received the Academy's silver medal and a full set of its memoirs. In 1819, when many of its maps were declared out of date, the trigonometric determinations of Wessel were made a special exception. In 1778 he passed an examination in Roman law. In 1815 he was made a Knight of the Danebrog. He died in 1818.

While Wessel was always well spoken of as a surveyor, he was never mentioned as a mathematician. Still the fact that his paper was the first to be accepted by the Academy from one not a member argues in his favor. This acceptance was due to Tetens, Councillor of State, to whom the MS. had been shown and whose assistance in improving it was acknowledged. In the History of the Academy of Sciences of Denmark, published in 1842, Professor Jürgensen classes Wessel with others in the statement, "The treatises of the other mathematicians are monographs of no considerable scientific value," or "They are too special to be discussed more at length."

In the introduction to his memoir Wessel says:

The present essay has for its object to determine how to express segments of straight lines when we wish by means of a unique equation between a single unknown segment and other given segments to find an

expression representing at once the length and direction of the unknown segment.

To be able to answer this question I shall employ two considerations which seem to me evident. In the first place, the variation of direction which may be produced by algebraic operations ought also to be represented by their symbols. In the second place we submit direction to algebra only by making its variation depend upon algebraic operations. Now according to the ordinary conception we can transform it by these operations only into the opposite direction, that is, from positive into negative and reciprocally. It follows that these two directions only would be susceptible of an analytic representation adapted to the usual conception and that the solution of the problem would be impossible for other directions. It is probably for this reason that nobody has given attention to this subject. Doubtless nobody has felt at liberty to change the definition of these operations once adopted. To this there is no objection so long as the definition is applied to ordinary quantities; but there are special cases where the peculiar nature of the quantities seems to invite us to give particular definitions to the operations. Then if we find these definitions advantageous it seems to me that we ought not to reject them. For in passing from arithmetic to geometric analysis, that is to say, from operations relative to abstract numbers to operations upon segments of a straight line, we shall have to consider quantities which may have to one another not only the same relations as abstract numbers, but also a great number of new relations Let us try then to generalize the signification of our operations; let us not restrict ourselves, as has been done hitherto, to the employment of segments of a straight line in the same or opposite senses, but extend a little the notion of the way in which they are applied not only to the same cases as heretofore, but to an infinite number of other cases. If at the same time that we take this liberty we have respect to the ordinary rules of operations we in no way contravene the ordinary theory of numbers, but we merely develop it, we accommodate ourselves to the nature of the quantities and observe the general rule which requires us to render a difficult theory little by little more easy to comprehend. It is not then absurd to demand that in geometry operations be taken in a broader sense than in arithmetic. We shall admit without difficulty that it will be possible to vary the direction of segments in an infinite number of ways. Precisely by this means (as we shall show later) we succeed not only in avoiding all impossible operations and in explaining the paradox that it is necessary sometimes to resort to the impossible to obtain the possible, but we also succeed in expressing the direction of line-segments situated in the same plane quite as analytically as their length, without the memoir being embarrassed by new symbols or new rules. Now it must be agreed that the general demonstration of geometric theorems often becomes easier when we express direction in an analytic manner and submit it to the rules of algebraic operations than when we are compelled to represent it by figures which are applicable only to particular cases.

For these reasons I have proposed to myself:

1° to give the rules of operations of this nature;

2° to show by examples the application to cases where the segments are found in the same plane;

3° to determine by a new method not algebraic the direction of segments situated in different planes;

4° to deduce the general solution of plane and spherical polygons;

5° to deduce in the same way the known formulæ of spherical trigonometry.

This, in brief, is an outline of the present memoir. I was led to write it by my desire to find a method which would enable me to avoid impossible operations; having discovered it I have made use of it to convince myself of the generality of certain known formulæ.

How well the author succeeds in carrying out his plan is shown by the memoir itself. Wessel says:

The addition of two segments is effected in the following manner: we combine them by drawing the one from the point where the other terminates; then we join by a new segment the two ends of the broken line thus determined.

He extends the definition to more than two segments and affirms:

In the addition of segments, the order of terms is arbitrary and the sum always remains the same.

His definition of the product of two segments is especially noteworthy:

The product of the two line-segments ought in every respect to be formed with one of the factors in the same way as the other factor is formed with the positive or absolute segment taken equal to unity; that is to say:

1° The factors ought to have such a direction that they can be placed in the same plane as the positive unit;

2° As to length the product should be to one of the factors as the other is to the unit;

3° As to the direction of the product, if we draw from the same origin the positive unit, the factors and the product, the latter ought to be in the plane of the unit and the factors, and ought to deviate from one of the factors by as many degrees and in the same sense as the other deviates from the unit so that the angle of direction of the product or its deviation with respect to the positive unit is equal to the sum of the angles of direction of the factors.

Let us designate by $+1$ the positive rectilinear unit, by $+\varepsilon$ another unit perpendicular to the first and having the same origin; then the

angle of direction of $+1$ will be equal to $0°$, that of -1 to $180°$, that of $+\varepsilon$ to $90°$ and that of $-\varepsilon$ to $-90°$ or to $270°$; and according to the rule that the angle of direction of the product is equal to the sum of the angles of the factors, we shall have: $(+1).(+1)=+1$, $(+1).(-1)=-1$, $(-1)\cdot(-1)=+1$, $(+1)\cdot(-\varepsilon)=-\varepsilon$, $(-1)\cdot(+\varepsilon)=-\varepsilon$, $(-1)(-\varepsilon)=+\varepsilon$, $(+\varepsilon)\cdot(+\varepsilon)=-1$, $(+\varepsilon).(-\varepsilon)=+1$, $(-\varepsilon)\cdot(-\varepsilon)=-1$. Hence it follows that ε is equal to $\sqrt{-1}$ and that the deviation of the product is determined so that we violate none of the rules of operation.

It is interesting to note that while Wessel makes the addition and multiplication of directed lines a matter of definition, Argand, in his famous memoir of 1806, *Essai sur une manière de représenter les quantités imaginaires dans les constructions géométriques*, says: "Inasmuch as these principles depend upon inductions which are not securely established, they cannot as yet be considered as other than hypotheses whose acceptance or rejection should depend upon either the consequences which they entail or a more rigorous logic;" although in his last contribution to the *Annales de Gergonne* he grants that this difficulty will vanish if, with M. Français, we define what is meant by a ratio of magnitude and position between two lines.

After explaining that if v represents any angle and $\sin v$ a segment equal in length to the sine, positive when the measuring arc terminates in the first semicircumference and negative when it terminates in the second, $\varepsilon \sin v$ will express the sine of the angle v in direction and magnitude, Wessel shows that any radius making the angle v with the positive unit will equal $\cos v + \varepsilon \sin v$. In the multiplication of two radii $\cos v + \varepsilon \sin v$, $\cos u + \varepsilon \sin u$, he establishes the distributive law by reference to the formulæ,

$$\sin(v+u) = \sin v \cos u + \cos v \sin u,$$
$$\cos(v+u) = \cos v \cos u - \sin v \sin u,$$

in contrast to Argand, who assumes the distributive law and then derives the trigonometric formulæ.

A statement in this connection is noteworthy:

But if we have to multiply line segments which are not both in the plane passing through the absolute unit we cannot apply the preceding rule. For this reason I do not consider the multiplication of such segments.

The treatment of division follows in a natural manner, and it is proved that indirect quantities share with direct quantities the property that if the dividend is a sum we obtain by dividing each term of the sum by the divisor several quotients whose sum is the quotient sought.

Then comes a discussion of powers and roots establishing the fact that $(\cos v + \varepsilon \sin v)^{\frac{1}{m}}$ has m different values and only m. In the next paragraph Wessel shows that the m^{th} power of a line-segment may be put in the form $e^{ma+mb\sqrt{-1}}$, where e^{ma} represents the length and mb the angle of direction, and that thus we have a new method of representing the direction of line-segments in the same plane by the aid of natural logarithms. This last is not again referred to, but it is readily seen that Wessel was in possession of all three of the present methods of representing the complex number,

$$a + b\sqrt{-1},\ r(\cos\varphi + \sqrt{-1}\sin\varphi) \text{ and } re^{\phi\sqrt{-1}}.$$

At the close of this section the author remarks:

At another time, with the permission of the Academy, I will present the complete proofs of these theorems. Having given an account of the way in which we must, in my judgment, understand the sum, the product, the quotient and power of line segments, I shall restrict myself to a few applications of the method.

The first application is to a demonstration of Cotes's theorem in which the fundamental theorem of algebraic equations is assumed as previously established. The second is to the resolution of plane polygons. In this certain characteristic notations occur. The first side of the quadrilateral considered is taken equal to the absolute unit; the sides in order beginning with the first are designated by the even numbers II, IV, VI, VIII, while I, III, V, VII, represent their deviations (in degrees), each with respect to the preceding side prolonged, regarding these deviations as positive or negative according as they have the same sense as the diurnal motion of the sun or the opposite; I′, III′, V′, VII′ denote the expressions $\cos \mathrm{I} + \varepsilon \sin \mathrm{I}$, etc., while I$^{-\prime}$, III$^{-\prime}$, V$^{-\prime}$, VII$^{-\prime}$ denote the expressions $\cos(-\mathrm{I}) + \varepsilon \sin(-\mathrm{I})$ or $\cos \mathrm{I} - \varepsilon \sin \mathrm{I}$, etc.

The author then deduces the two formulæ,

$$\mathrm{II} + \mathrm{IV}\ \mathrm{III}' + \mathrm{VI}\cdot\mathrm{III}'\cdot\mathrm{V}' + \mathrm{VIII}\cdot\mathrm{III}'\cdot\mathrm{V}'\cdot\mathrm{VII}' = 0,$$
$$\mathrm{II}\cdot\mathrm{III}'\cdot\mathrm{V}'\ \mathrm{VII}' + \mathrm{IV}\cdot\mathrm{V}'\cdot\mathrm{VII}' + \mathrm{VI}\cdot\mathrm{VII}' + \mathrm{VIII} = 0,$$

and proves that two equations of this form will suffice for the solution of any polygon in which the only unknown parts are three angles, or two angles and a side, or an angle and two sides.

Wessel next attacks the problem of representing the direction of any line segment in space by taking it as the radius, r, of a sphere. Assuming three perpendicular radii as axes and denoting positive unit lengths upon these, to the left by 1, forward by ϵ and upward by η respectively, where $\epsilon^2 = -1$, and $\eta^2 = -1$, he concludes that a radius whose extremity has for coördinates x, $\eta\, y$, $\epsilon\, z$ will be properly designated by $x + \eta\, y + \epsilon\, z$. Defining the plane of r and ϵr as the horizontal plane and that of r and ηr as the vertical plane, he examines the effect of moving the extremity through an arc of I degrees parallel to the horizontal plane and obtains for $x + \eta y + \epsilon z$ the new value,

$$\eta y + (x + \epsilon z)\ \ (\cos \mathrm{I} + \epsilon \sin \mathrm{I}) = \eta y + x \cos \mathrm{I} - z \sin \mathrm{I} + \epsilon x \sin \mathrm{I} + \epsilon z \cos \mathrm{I},$$

in which the term ηy remains unchanged. This operation he indicates by the use of the sign ,, as $(x + \eta y + \epsilon z)$,, $(\cos \mathrm{I} + \epsilon \sin \mathrm{I})$ and says that it has only imperfectly the signification of a sign of multiplication, for the operation leaves unchanged that one of the segments occurring in the multiplicand which is outside of the plane corresponding to the rotation indicated by the multiplier. He calls attention to the fact that the factors must be used in order from left to right. Similarly when the extremity of the radius moves through an arc of II degrees parallel to the vertical plane we have

$$(x + \eta y + \epsilon z)\ ,,\ (\cos \mathrm{II} + \eta \sin \mathrm{II}) = \epsilon z + x \cos \mathrm{II} - y \sin \mathrm{II} + \eta x \sin \mathrm{II} + \eta y \cos \mathrm{II}.$$

It follows at once that

$$(x + \eta y + \epsilon z)\ ,,\ (\cos \mathrm{I} + \epsilon \sin \mathrm{I})\ ,,\ (\cos \mathrm{III} + \epsilon \sin \mathrm{III}) = (x + \eta y + \epsilon z)\ ,,\ (\cos (\mathrm{I} + \mathrm{III}) + \epsilon \sin (\mathrm{I} + \mathrm{III})\ )$$

and

$$(x + \eta y + \epsilon z)\ ,,\ (\cos \mathrm{II} + \eta \sin \mathrm{II})\ ,,\ (\cos \mathrm{IV} + \eta \sin \mathrm{IV}) = (x + \eta y + \epsilon z)\ ,,\ (\cos (\mathrm{II} + \mathrm{IV}) + \eta \sin (\mathrm{II} + \mathrm{IV})\ )$$

also that

$$x + \eta y + \epsilon z = (x + \eta y + \epsilon z)\ ,,\ (\cos \mathrm{I} + \epsilon \sin \mathrm{I})\ ,,\ (\cos \mathrm{I} - \epsilon \sin \mathrm{I}) = (x + \eta y + \epsilon z)\ ,,\ (\cos \mathrm{II} + \eta \sin \mathrm{II})\ ,,\ (\cos \mathrm{II} - \eta \sin \mathrm{II}).$$

Wessel then studies the effect of alternate horizontal and vertical rotations. Representing the radius in its first position by s and in its final position by S, and denoting the arcs in order by I, II, III, . . . VI, he obtains the formula

$$S = s \,,,\, \mathrm{I}' \,,,\, \mathrm{II}' \,,,\, \mathrm{III}' \,,,\, \mathrm{IV}' \,,,\, \mathrm{V}' \,,,\, \mathrm{VI}'.$$

In this connection he observes that such factors as V′., VI′ can be transferred to the first member by using their reciprocals in inverse order, as

$$S \,,,\, \mathrm{VI}^{-\prime} \,,,\, \mathrm{V}^{-\prime} \,,,\, \mathrm{IV}^{-\prime} = s \,,,\, \mathrm{I}' \,,,\, \mathrm{II}' \,,,\, \mathrm{III}' \,,,\, .$$

These results are applied to the solution of spherical polygons and the determination of the properties of spherical triangles. As in the case of plane polygons, I, II, III, etc., represent the exterior angles and sides in order, the odd numbers the angles, and the even numbers the sides. Supposing the angles and the sides of a polygon known except one angle and two sides, or two angles and a side, or three angles, or three sides, the unknown parts can be determined by the equation

$$s \,,,\, \mathrm{I}' \,,,\, \mathrm{II}' \,,,\, \mathrm{III}' \,,,\, \mathrm{IV}' \,,,\, \mathrm{V}' \,,,\, \mathrm{VI}' \,,,\, \ldots \,,,\, N' = s,$$

where s is indeterminate, and may be supposed equal to r, ϵr, or ηr. The effect of the rotations indicated by this equation is to submit the sphere alternately to rotations about the axis of the horizon and the axis of the vertical circle so that each point of the sphere describes first a horizontal arc which measures the first exterior angle of the polygon, then a vertical arc containing as many degrees as the first side of the polygon, then a new horizontal arc which measures the second angle, etc. The sphere finally returns to its original position, while each of its points has described as many horizontal arcs as the polygon has angles and as many vertical arcs as it has sides.

While Wessel's results, as obtained by these alternate rotations, are correct so far as they go, he fails to observe that a general rotation must be compounded of three rotations about the axes ϵ, η, ϵ or η, ϵ, η. Stranger still he makes no study of rotations about the real axis. Thiele, in his introduction to Wessel's memoir, shows how easy it would have been to go a few steps further and arrive at the notion of quaternions. But be that as it may, Wessel deserves great credit for having devised the only suc-

cessful method of dealing with line-segments in space previous to the work of Hamilton beginning in 1843.

Unmindful of Euler's demonstration of the real value of

$$(\sqrt{-1})^{\sqrt{-1}}$$

Argand endeavors to show that such an expression may be used to represent a directed line in space. Français tries to solve the problem by the use of imaginary angles, but frankly acknowledges his failure. Servois sees with remarkable clearness what is needed, but is unable to reach it. He says:

The table of double argument which you (Gergonne) propose, as applied to a plane supposed to be so divided into points or *infinitesimal* squares that each square corresponds to a number which would be its *index*, would very properly indicate the length and position of the radii vectores which revolve about the point or central square corresponding to ± 0; and it is quite remarkable that if we designated the length of a radius vector by a, and the angle it makes with the real line. . . . . ., -1, ± 0, $+1$. . . . . by a, the rectangular coördinates of its *extremity remote from the origin* by x, y, the real line being the axis of x. the point would be determined by $x+y\sqrt{-1}$. . . . . . It is clear that your ingenious tabular arrangement of numerical magnitudes may be regarded as a central slice (*tranche centrale*) of a table of triple argument representing points and lines in tri-dimensional space. You will doubtless give to each term a trinomial form; but what would be the coëfficient of the third term? For my part I cannot tell. Analogy would seem to indicate that the trinomial should be of the form $p\cos\alpha+q\cos\beta+r\cos\gamma$, α, β, and γ being the angles made by a right line with three rectangular axes and that we should have

$$(p\cos\alpha + q\cos\beta + r\cos\gamma)\ (p'\cos\alpha + q'\cos\beta + r'\cos\gamma)$$
$$= \cos^2\alpha + \cos^2\beta + \cos^2\gamma = 1.$$

The values of p, q, r, p', q', r' satisfying this condition would be *absurd*, but would they be imaginaries, reducible to the general form $A+B\sqrt{-1}$?

As we all know now, these non-reals which Servois could not determine may be identified with the $+i, +j, +k, -i, -j, -k$, of Hamilton's Quaternions.

In 1779, in his first published paper, *Demonstratio nova theorematis omnem functionem algebraicam rationalem integram unius variabilis in factores reales primi vel secundi gradus resolvi posse*, the celebrated Gauss, then only twenty-two years of age, says:

By an imaginary quantity I always understand here a quantity contained in the form $a+b\sqrt{-1}$, so long as b is not zero. * * * If imaginary

quantities are to be retained in analysis (which for many reasons seems better than to abolish them, provided they are established on a sufficiently solid foundation) it is necessary that they be considered as equally possible with real quantities, on which account I should prefer to include both real and imaginary quantities under the common designation *possible quantities*. * * * A vindication of these (*i. e.*, imaginary quantities), as well as a more fruitful exposition of the whole matter, I reserve for another occasion.

This occasion, however, does not seem to have come till more than thirty years later. In the *Göttingische gelehrte Anzeigen* of April 23, 1831, in an account by Gauss of his own paper *Theoria residuorum biquadraticorum, Commentatio secunda*, we read:

Our general arithmetic, so far surpassing in extent the geometry of the ancients, is entirely the creation of modern times. Starting originally from the notion of absolute integers, it has gradually enlarged its domain. To integers have been added fractions, to rational quantities the irrational, to positive the negative and to the real the imaginary. This advance, however, has always been made at first with timorous and hesitating step. The early algebraists called the negative roots of equations false roots, and these are indeed so when the problem to which they relate has been stated in such a form that the character of the quantity sought allows of no opposite. But just as in general arithmetic no one would hesitate to admit fractions, although there are so many countable things where a fraction has no meaning, so we ought not to deny to negative numbers the rights accorded to positive simply because innumerable things allow no opposite. The reality of negative numbers is sufficiently justified since in innumerable other cases they find an adequate substratum. This has long been admitted, but the imaginary quantities — formerly and occasionally now, though improperly, called impossible — as opposed to real quantities are still rather tolerated than fully naturalized, and appear more like an empty play upon symbols to which a thinkable substratum is unhesitatingly denied by those who would not depreciate the rich contribution which this play upon symbols has made to the treasure of the relations of real quantities.

The author has for many years considered this highly important part of mathematics from a different point of view, where just as objective an existence may be assigned to imaginary as to negative quantities, but hitherto he has lacked opportunity to publish these views, though careful readers may find traces of them in the memoir upon equations which appeared in 1799 and again in the prize memoir upon the transformation of surfaces. In the present paper the outlines are given briefly; they consist of the following:

Positive and negative numbers can only find an application when the thing counted has an opposite which when conceived of as united with it has the effect of destroying it. Accurately speaking, this supposition can

only be made where the things enumerated are not substances (objects thinkable in themselves), but relations between any two objects. It is postulated that these objects are arranged after a definite fashion in a series, *e. g* , A, B, C, D, * * * and that the relation of A to B can be regarded as equal to that of B to C, etc. The notion of opposition involves nothing further than the *interchange* of the terms of the relation so that if the relation of (or transition from) A to B is considered as $+1$ the relation of B to A must be represented by -1. So far then as such a series is unlimited on both sides, every real integer represents the relation of a term arbitrarily taken as origin to a definite term of the series.

If, however, the objects are of such a kind that they cannot be arranged in one series, even though unlimited, but only in series of series, or, what amounts to the same thing, they form a manifoldness of two dimensions; if there is the same connection between the relations of one series to another, or the transitions from one to another, as in the case of the transition from one term of a series to another term of the same series, we shall evidently need for the measurement of the transition from one term of the system to another, besides the previous units $+1$ and -1, two others opposite in character $+i$ and $-i$. Obviously we must also postulate that the unit i shall always mark the transition from a given term of the one series to a definite term of the immediately adjacent series. In this way the system can be arranged in a two-fold manner in series of series.

The mathematician leaves entirely out of consideration the nature of the objects and the content of their relations. He has simply to do with the enumeration and comparison of the relations. So far as he has assigned sameness of nature to the relations designated by $+1$ and -1, considered in themselves, he is warranted in extending such sameness to all four elements $+1$, -1, $+i$, $-i$.

These relations can be made intuitive only by a representation in space and the simplest case, where there is no reason for arranging the objects in any other than quadratic fashion, is that in which an unlimited plane is divided into squares by two systems of parallel lines intersecting at right angles, and the points of intersection are selected as the symbols. Every such point has four adjacent points, and if we designate the relation A to a neighboring point by $+1$, the relation to be denoted by -1 is determined of itself, while we can select which of the two others we please for $+i$, or can take the point to be denoted by $+i$ at pleasure on the *right* or *left*. This distinction between right or left so soon as we have fixed (at pleasure) upon forwards and backwards *in* the plane, and above and below with respect to the two sides of the plane is completely determined *in itself*, although we can convey our own intuition of this difference to others *only* by reference to actually existent material things. But when we have decided upon the latter we see that it is still a matter of choice as to which of the two series intersecting at one point we shall regard as the principal series and which direction in it shall be considered as having to do with positive numbers. We see further that if we wish

to take $+1$ for the relation previously expressed by $+i$, we must necessarily take $+i$ for the relation previously expressed by -1. In the language of mathematicians this means that $+i$ is *a* mean proportional between $+1$ and -1, or corresponds to the symbol $\sqrt{-1}$. We say purposely not *the* mean proportional because $-i$ has just as good a right to that designation. Here then the demonstrability of an intuitive signification of $\sqrt{-1}$ has been fully justified and nothing more is necessary to bring this quantity into the domain of objects of arithmetic.

We have thought to render the friends of mathematics a service by this brief exposition of the principal elements of a new theory of the so-called imaginary quantities. If people have considered this subject from a false point of view and thereby found a mysterious obscurity, this is largely due to an unsuitable nomenclature. If $+1$, -1, $\sqrt{-1}$ had not been called positive, negative, imaginary (or impossible) unity, but perhaps direct, inverse, lateral unity, such obscurity could hardly have been suggested. The subject which, properly enough, in the present treatise has been touched upon only incidentally, the author has reserved for a more elaborate treatment in the future, where also the question will be answered as to why the relations between things which present a manifoldness of more than two dimensions cannot furnish still other classes of magnitudes admissible in general arithmetic.

Such was Gauss's masterly presentation of the underlying principles of the treatment of the imaginary. In Germany the impulse given by his commanding influence is felt even to the present day.

Buée's memoir *Sur les Quantités Imaginaires*, read before the Royal Society of London in 1805 and covering sixty-five pages of the Philosophical Transactions of 1806, is somewhat vague and disappointing. He describes $\sqrt{-1}$ as follows:

$\sqrt{-1}$ is the sign of perpendicularity. $\sqrt{-1}$ is not the sign of an arithmetical operation, nor of an arithmetico-geometric operation, but of an operation purely geometric. It is a purely descriptive sign which indicates the direction of a line without regard to its length.

Near the close of his paper he investigates what becomes of the conic sections when their coördinates become imaginary and decides that the circle passes into an equilateral hyperbola in the plane perpendicular to the plane of the circle and similarly for the other conics.

A further discussion of the justly celebrated epoch-making memoir of Argand and the contributions of himself, Français, Gergonne and Servois to the *Annales de Gergonne* from 1813 to 1815 is rendered the less necessary by reason of Houel's republication

of all these papers in 1874 and their translation into English by Hardy in 1881.

It is interesting to note the early view of imaginaries entertained by so distinguished a mathematician as Cauchy. In his *Cours d'Analyse*, 1821, we read:

In analysis we apply the term symbolic expression or symbol to every combination of algebraic signs which signifies nothing by itself or to which we attribute a value different from that which it naturally ought to have. * * * * Among the symbolic expressions whose consideration is of importance in analysis we ought especially to distinguish those which are called imaginary. * * * * We write the formula

$$\cos(a+b)+\sqrt{-1}\sin(a+b)=(\cos a+\sqrt{-1}\sin a)(\cos b+\sqrt{-1}\sin b].$$

The three expressions which the preceding equation contains * * * * are three symbolic expressions which cannot be interpreted according to generally established conventions and represent nothing real. * * * * The equation itself, strictly speaking, is inexact and has no meaning.

In 1849, however, in a paper *Sur les quantités géométriques*, in which he gives suitable credit to Argand, Français and others, he acknowledges:

In my *Analyse algébrique*, published in 1821, I was content to show that the theory of imaginary expressions and equations could be rendered rigorous by considering these expressions and equations symbolic. But after new and mature reflections the better side to take seems to be to abandon entirely the use of the sign $\sqrt{-1}$ and to replace the theory of imaginary expressions by the theory of quantities which I shall call geometric.

Having defined the term geometric quantity exactly as we now define the term vector and shown when two geometric quantities are equal, he continues:

The notion of *geometric quantity* will comprehend as a particular case the notion of *algebraic quantity*, positive or negative, and *a fortiori* the notion of *arithmetic quantity*. * * * We must further define the different functions of these quantities, especially their sums, their products and their integral powers by choosing such definitions as agree with those admitted when we are dealing with algebraic quantities alone. This condition will be fulfilled if we adopt the conventions now to be given.

Then follow the definitions called for, together with a treatment of the whole subject fully up to modern demands. Cauchy observes that a large part of the results of the investigations of Argand and others would seem to have been discovered as early

as 1786 by Henri Dominique Truel, who communicated them about 1810 to Augustin Normand, of Havre.

In 1828 there appeared in Cambridge, England, a remarkable work by Rev. John Warren, entitled *A Treatise on the Geometrical Representation of the Square Roots of Negative Quantities.* Though this book has latterly received scant credit, its merits were fully recognized by De Morgan and acknowledgments of indebtedness were frankly made by Hamilton.

Throughout Warren's work the term quantity, like Cauchy's geometric quantity, indicates a line given in length and direction. Some of his definitions are as follows:

The sum of two quantities is the diagonal of the parallelogram whose sides are the two quantities. The first of four quantities is said to have to the second the same ratio which the third has to the fourth; when the first has *in length* to the second the same ratio which the third has *in length* to the fourth, according to Euclid's definition; and also the angle at which the fourth is inclined to the third is equal to the angle at which the second is inclined to the first, and is measured in the same direction. Unity is a positive quantity arbitrarily assumed from a comparison with which the values of other quantities are determined. If there be three quantities such that unity is to the first as the second to the third, the third is called the *product*, which arises from the *multiplication* of the second by the first. If there be three quantities such that the first is to unity as the second is to the third, the first quantity is called the *quotient*, which arises from the *division* of the second by the third.

The fundamental laws of algebra as governing these quantities are established in their utmost generality with a rigor of reasoning that has probably not been surpassed. The author even goes so far as to deduce the binomial formula, to develop many series and to apply the methods of the differential and integral calculus to quantities of the class defined. In form Warren's work is intensely algebraic and fairly bristles with formulæ.

To sum up:

Caspar Wessel, in 1797, published the first clear, accurate and scientific treatment of directed lines in the same plane, as represented by quantities of the form $a + b\sqrt{-1}$, establishing the laws governing their addition, subtraction, multiplication and division, and showing these quantities to be of practical value in the demonstration of theorems and solution of problems; he also worked out a partial theory of rotations in space, so far as they can be decomposed into rotations about two axes at right angles.

Not very much later, 1799, Gauss indicated that he was in possession of a method of dealing with quantities of the form $a + b\sqrt{-1}$ which would consider them as equally possible with real quantities, but its fuller exposition was deferred till 1831.

Buée's paper of 1805 lays great emphasis upon $\sqrt{-1}$ as the sign of perpendicularity, but fails to give any satisfactory interpretation of the product of directed lines.

Argand's famous memoir of 1806 is hardly in danger of receiving too much credit. Though written after Wessel's paper there is not the slightest probability that Argand had any knowledge of the Norwegian surveyor and, in fact, certain of his theorems are established less rigorously than by Wessel. Argand gave numerous applications of his theory to trigonometry, geometry and algebra, some of which are very noteworthy, especially his demonstrations of Ptolemy's theorem regarding the inscribed quadrilateral and of the fundamental proposition of the theory of equations.

The contributions of Français, Gergonne and Servois, 1813–1815, served to do away with some of the errors into which Argand had fallen and thus to give a clearer insight into the fundamental notions of the subject.

Though Warren's book of 1828 contains definitions differing but little from those of Wessel and Français and a notation which seems only a modification of that of Français, his generalized treatment of directed lines in the plane must be regarded as highly original.

Cauchy's work lay in the extension and development of the labors of his predecessors rather than in the introduction of new ideas.

Such were the beginnings of the study of the geometric representation of the imaginary which has led in modern times to the establishment of such great bodies of doctrine as the theory of functions on the one side and quaternions on the other, with the Ausdehnungslehre occupying a position between. Who can tell what the next century will bring forth?

PAPERS READ.

[ABSTRACTS.]

ON THE SIMPLE ISOMORPHISMS OF A SUBSTITUTION GROUP TO ITSELF. By Dr. G. A. MILLER, Chicago, Ill.

A GREAT part of the difficulties connected with the study of groups resides either in the simple groups or in the general problem of isomorphisms. To every simple isomorphism of a group to itself corresponds some substitution of its operators. To all such isomorphisms corresponds a substitution group, which has been called the *group of isomorphisms* of the given group.

When a regular group (R) of order n is transformed into itself by the largest possible group (L) of its own degree, the subgroup of L which includes all its substitutions that do not contain a given element is the group of isomorphisms of R. This is evident when we suppose that all the substitutions of R begin with the given element and are represented by their second elements. Hence there are just n substitutions that are commutative to every substitution of R and contain no element that is not found in R. This important theorem is due to Jordan. The preceding method of proving it appears to be simple and new.

Isomorphisms which may be derived from a given one by means of real transforming operators may be said to be *transform* with respect to each other. All simple isomorphisms of R to itself are transforms of the identical isomorphism. This is also true in regard to operation groups but not in regard to the general substitution groups. Isomorphisms that do not belong to the same system of transforms are said to be *distinct*. To the transforms of the identical isomorphisms corresponds a subgroup of the group of isomorphisms. The number of substitutions of this group that correspond to an isomorphism which is distinct from the identical is a multiple of the order of this subgroup.

CONTINUOUS GROUPS OF SPHERICAL TRANSFORMATIONS IN SPACE. By Prof. H. B. NEWSON, Lawrence, Kans.

[*Communicated by the Secretary.*]

THE general group is the group of Lie's Kugelgeometrie. In this development all infinity in space is regarded as a single point as in the complex plane. All planes are regarded as spheres through the point at

infinity. The transformations are conformal, preserving angles. The general group is tenfold. All transformations leaving a point invariant form a sevenfold subgroup. Whenever the point at infinity is invariant the transformation is projective since it transforms planes into planes. There is a sixfold subgroup which leaves a sphere invariant. Also the same for a plane. This sixfold group is identical with the sixfold group of circular transformations on the Neumann sphere or in the complex plane, and hence it is expressed by the transformations of the complex variable.

[This paper will be printed in the Bulletin of the American Mathematical Society.]

THE TREATMENT OF DIFFERENTIAL EQUATIONS BY APPROXIMATE METHODS. By Prof. W. F. DURAND, Ithaca, N. Y.

IN this paper methods are shown for the approximate treatment of such differential equations as do not admit of solution by analytical processes. The values at an initial point are supposed to be known, and from this point the values at successive points are found by a step by step process.

Denoting the points by subscripts 0, 1, 2, etc., and successive derivatives of y with reference to x by y' y'', etc., we may, for example, by the simple trapezoidal rule, express approximately the difference between y'_0 and y'_1 in terms of y''_0 and y''_1, and similarly that between y_0 and y_1 in terms of y'_0 and y'_1. Hence y'_1 and y_1 may be thus ultimately expressed in terms of y_0, y'_0, y''_0 and y''_1. We may then substitute these values in the differential equation, consider y''_1 as the unknown quantity, and if linear, solve directly. The resulting value substituted back will give values of y'_1 and y_1, and hence the quantities for this point are completely known. If the equation is not linear it may be solved by tentative or approximate means, and thus the values for the point (1) found in the same general manner. The values thus determined provide a new point of departure from which similarly the values for the next point are found, and thus onward step by step.

By the use of other and more accurate rules for approximate integration, we may involve three or more ordinates and thus step from (0) and (1) to (2), and from (1) and (2) to (3), and so on; and similarly for rules of still higher orders.

It is furthermore pointed out that these methods are peculiarly valuable in cases where the equation involves graphical representations of variables not expressible by analytical means, as in various physical problems.

These methods are, of course, independent of the order of the equation, and those of the third or fourth order may be quite as readily treated as those of the second or first, except for the greater amount of work involved in carrying out the operations.

In like manner equations involving complex quantities may be treated by the same methods, and with no added difficulty aside from the additional operative work introduced.

The application of these methods to simultaneous equations is also pointed out, and the paper concludes with several problems illustrating the methods discussed, and showing the nature of their application.

[This paper will be published in the Annals of Mathematics.]

COMMUTATIVE MATRICES. By Prof. JAMES BYRNIE SHAW, Illinois College, Jacksonville, Ill.

1. IF the latent roots of the matrix φ, order n. are $g_1, g_2, \ldots$ of multiplicity $m_1, m_2, \ldots$ respectively, then φ may be written

$$\varphi = \varphi_1{}' + \varphi_2{}'' + \varphi_3{}''' + \ldots$$

where $\varphi_1{}'$ is a matrix of order m_1, whose roots are all equal to g_1, and which operates on an extension of order m_1; $\varphi_2{}''$ is a matrix of order m_2, roots equal to g_2, operating on an extension of order m_2, etc. The extensions spoken of are mutually exclusive and together make up the entire extension n upon which φ operates (*Taber*, *Amer. Jour. Math.* Vol. 12, p. 373). We have

$$\begin{aligned} \varphi_1{}' \, \varphi_2{}'' &= 0 \text{ etc.} \\ \varphi_1{}^2 &= \varphi_1{}' \text{ etc.} \end{aligned}$$

2. Any matrix ψ may be expressed in terms of a series of matrices thus

$$\begin{aligned} \psi = \psi_1{}' &+ \psi_2{}'' + \ldots \\ + \psi_2{}' &+ \psi_3{}' + \ldots \\ + \psi_1{}'' &+ \psi_3{}'' + \ldots \\ + \psi_1{}''' &+ \psi_2{}''' + \ldots \\ \ldots & \ldots \end{aligned}$$

where the matrix $\psi^{(r)}_s$ converts any vector in the extension belonging to $\varphi^{(s)}_s$ into a vector in the extension belonging to $\psi^{(r)}_r$. That is

$$\psi = \Sigma b_{rs} \, \eta^{(r)}_s$$

where $\eta^{(r)}_s$ converts the defining unit vector ρ_s into the unit vector ρ_r.

3. Now if ψ is commutative with φ,

$$\varphi \psi = \psi \varphi.$$

Expanding both sides we have a set of linear homogeneous equations in the b's as many in number as the unknown b's. The determinants of

certain ones of this set do not vanish, hence those b's are each 0, namely, those belonging to $\psi_2', \psi_3', \ldots \psi_1'', \psi_3'', \ldots$ Hence

$$\psi = \psi_1' + \psi_2'' + \psi_3''' + \ldots$$

4. Therefore, if φ and ψ are commutative with each other, either is the sum of a series of partial matrices, each partial matrix having a unique ground, and equal roots; and the other is the sum of a series of corresponding partial matrices which have the same unique grounds, but whose roots may not be equal.

5. The consideration of the restrictions on the equality of the roots of the partial matrices of the second matrix will be taken up elsewhere.

[This paper will be published in American Journal of Mathematics.]

ON THE THEORY OF THE QUADRATIC EQUATION. By Prof. A. MACFARLANE, Lehigh University, South Bethlehem, Pa.

CONSIDER the quadratic equation

$$x^2 + 2bx + c = 0$$

with solution

$$x = -b \pm \sqrt{b^2 - c}\,.$$

The current theory may be stated as follows: When b^2 is greater than c, the radical term is to be simply added to the other term, and is represented as part of the same straight line; but when b^2 is less than c, then the radical $\sqrt{c - b^2}$ is to be geometrically added to the other term, and is represented as at right angles to it. This theory is inadequate, and requires to be extended as follows. When b^2 is greater than c, the radical may be represented at right angles, so that the two terms of the root represent a hyperbolic complex quantity: this is the interpretation which is supplementary to the circular complex quantity; in both cases x is a complex quantity. Again when b^2 is less than c, x may be a scalar quantity; in which case the two parts of the root are represented along the same straight line. The cosine of an angle which is the sum of a circular and a complex angle is of this character.

ON A NEW PRINCIPLE IN SOLVING CERTAIN LINEAR DIFFERENTIAL EQUATIONS WHICH OCCUR IN MATHEMATICAL PHYSICS. By Prof. A. MACFARLANE, South Bethlehem, Pa.

WHEN the right hand member is the orthogonal projection of a plane motion, it is easier to pass to the auxiliary motion using plane algebra

than it is to proceed with the given equation directly. For example, let the differential equation be

$$\frac{di}{dt} + \frac{R}{L}i = \frac{E}{L}\sin \omega t$$

where R, L, E, and i are scalar quantities. When we substitute $e^{\sqrt{-1}\,\omega t}$ instead of $\sin \omega t$ we pass to plane algebra, and the variable is then planar and will be denoted by I. Hence

$$\frac{dI}{dt} + \frac{R}{L}I = \frac{E}{L}e^{\sqrt{-1}\,\omega t}$$

As the rules for plane algebra are the same as those for linear algebra, we derive

$$I = \frac{E}{L}e^{-\frac{Rt}{L}}\int e^{\frac{Rt}{L}}e^{\sqrt{-1}\,\omega t} \qquad + Ce^{-\frac{R}{L}t}$$

$$= \frac{E}{L}e^{-\frac{Rt}{L}}\int e^{\left(\frac{Rt}{L}+\sqrt{-1}\,\omega\right)t} \qquad + Ce^{-\frac{R}{L}t}$$

$$= \frac{E}{L}\,\frac{e^{-\frac{Rt}{L}+\frac{Rt}{L}+\sqrt{-1}\,\omega t}}{\frac{R}{L}+\sqrt{-1}\,\omega} \qquad + Ce^{-\frac{R}{L}t}$$

$$= \frac{E}{L}\,\frac{e^{\sqrt{-1}\,\omega t}\,e^{-\sqrt{-1}\tan^{-1}\frac{L\omega}{R}}}{\sqrt{\frac{R^2}{L^2}+\omega^2}} \qquad + Ce^{-\frac{R}{L}t}$$

$$= \frac{E}{\sqrt{R^2+(L\omega)^2}}\,e^{\sqrt{-1}\left(\omega t-\tan^{-1}\frac{L\omega}{R}\right)} \qquad + Ce^{-\frac{R}{L}t}$$

Therefore

$$i = \frac{E}{\sqrt{R^2+(L\omega)^2}}\sin\left(\omega t - \tan^{-1}\frac{L\omega}{R}\right) \qquad + Ce^{-\frac{Rt}{L}}$$

This method was used in a paper which I read before the International Electrical Congress at Chicago (Trans. Amer. Inst. Elect. Engineers, Vol. x, p. 186).

CONDITION THAT THE LINE COMMON TO N—1 PLANES IN AN N-SPACE MAY PIERCE A GIVEN QUADRIC SURFACE IN THE SAME SPACE. By Dr. VIRGIL SNYDER, Ithaca, N. Y.

[*Communicated by the Secretary.*]

GIVEN $n-1$ linear equations

$$\sum_{i=1}^{n+1} a_{ik}\, x_i = 0 \qquad [k = 1, 2, \quad . \quad . \quad . \quad n-1$$

and one quadratic equation

$$\varphi\,(x_1, x_2 \ldots x_6) = \varphi\,(x) = 0,$$

homogeneous in the same $n + 1$ variables

$$x_1,\ x_2,\ \ldots\ x_{n+1}$$

The problem is to determine when the simultaneous values of $x_1 \ \ldots \ x_{n+1}$ that satisfy these equations shall be real.

When $a_{ik} = \dfrac{\delta\varphi\,(x)}{\delta x_i}$, then $x_i = \dfrac{\delta\,\Phi\,(a_{1k},\ a_{2k}\ \ldots\ a_{6k})}{\delta a_{ik}}$ where

$$\Phi\ (a_{1k}\ a_{2k}\,,\ \ldots\ a_{6k}) = \Phi\ (a_k) = 0$$

is the condition that the plane $\sum_{i=1}^{n+1} a_{ik}\, x_i = 0$ may touch the quadric $\varphi\,(x) = 0$.

Let $\Phi\ (a_k,\ a_l)$ represent the polar of the forms $\Phi\ (a_k)$ and $\Phi\ (a_l)$ such that

$$\Phi\ \left(a_k,\ a_l\right) = \sum_{i=1}^{n+1} a_{ik} \frac{\delta\,\Phi\,(a_l)}{\delta\,a_{il}} = \Phi\ \left(a_l,\ a_k\right).$$

The following theorem expresses the result:

The simultaneous values are real, coincident or imaginary, according as $(-1)^{h+1}$. *H is positive, zero or negative;* where H stands for the determinant

$$H = \begin{vmatrix} \Phi\left(a_1\right), & \Phi\left(a_1, a_2\right), \ \ldots & \Phi\left(a_1, a_{n+1}\right) \\ \Phi\left(a_2, a_1\right), & \Phi\left(a_2\right), \ \ldots\ldots & \Phi\left(a_3, a_{n+1}\right) \\ \Phi\left(a_3, a_1\right), & \Phi\left(a_3, a_2\right) \ldots\ldots & \Phi\left(a_2, a_{n+1}\right) \\ \ldots\ldots & \ldots\ldots\ldots & \ldots\ldots \\ \Phi\left(a_{n+1}, a_1\right), & \Phi\left(a_{n+1}, a_2\right), \ \ldots & \Phi\left(\quad a_{n+1}\right) \end{vmatrix}$$

and h is the number of negative terms that appear in $\varphi(x)$, after being transformed by a real substitution into the algebraic sum of squares.

The criterion is of equal value whether n is odd or even, and has practical applications for $n = 2, 3, 4$ or 5, in point, line and spherical geometry.

[To be published in The Bulletin of the American Mathematical Society.]

THE PSYCHOLOGY OF THE PERSONAL EQUATION. By Prof. T. H. SAFFORD, Williamstown, Mass.

THE object of this paper is to awaken an interest among astronomers and psychologists such as to induce them to pay more attention to each other's work, and thus improve their own methods where necessary.

[This paper is printed in Science, September, 1897.]

COMPOUND DETERMINANTS. (PRELIMINARY COMMUNICATION.) By Prof. W. H. METZLER, Syracuse University, Syracuse, N. Y.

IN this paper the idea of obtaining the value of $A_{(m)}$ the m^{th} compound of the determinant A as a power of A, by multiplying it by its adjugate $A_{(n\text{-}m)}$, the $(n\text{-}m)^{\text{th}}$ compound of A, is extended to finding the value of certain minors of $A_{(m)}$ in terms of A and its minors. Taking any minor of A and forming its m^{th} and $(n\text{-}m)^{\text{th}}$ compounds their values in terms of the minor is in the same way found. Making use of this principle and the laws of *Complementaries and Extensible Minors*, it is shown how the value of many minors of $A_{(m)}$ may be found; and, by making use of a comprehensive notation, the whole subject is unified, the laws of vanishing minors set forth and such well known theorems as Sylvester's and others are easily established.

[This paper will be published in American Journal of Mathematics.]

WATERS WITHIN THE EARTH AND LAWS OF RAINFLOW. By W. S. AUCHINCLOSS, C.E., Philadelphia, Pa.

[This paper has been printed by the author for distribution.]

ON THE SECULAR MOTION OF THE EARTH'S MAGNETIC AXIS. By Dr. L. A. BAUER, University of Cincinnati, Cincinnati, O.

ABOUT 70 % of the total magnetization of the earth can be referred to a homogeneous magnetization about a diameter inclined to the earth's rotation axis by an angular amount of about 12°. This ax s has been termed by Gauss the earth's magnetic axis. It is an interesting question to determine the motion of this axis during the past two or three centuries. The present paper is an attempt to solve this problem as far as is possible with the data at present at our command.

[This paper will be published in Terrestrial Magnetism.]

SIMPLE EXPRESSIONS FOR THE DIURNAL RANGE OF THE MAGNETIC DECLINATION AND THE MAGNETIC INCLINATION. By Dr. L. A. BAUER, University of Cincinnati, Cincinnati, O.

As yet no formulæ had been found by which the diurnal range of the magnetic declination, for example, could be computed for various portions of the earth. The writer has found the following most simple formulæ to hold true within the fluctuations to which the quantities themselves are subject:

$$\text{Diurnal range of declination} = 2'.58 \operatorname{Sec}^2 \varphi \quad . \quad . \quad . \quad (1)$$

$$\text{" \quad " \quad " inclination} = \frac{6'.1}{1 + 3 \sin^2 \varphi} \quad . \quad . \quad . \quad (2)$$

where φ = magnetic latitude is found from the equation

$$\tan \varphi = \tfrac{1}{2} \tan I \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Equation (1) was found in the first place empirically, then under certain assumptions deduced theoretically. Equation (2) was then deduced theoretically and found to satisfy the data.

[This paper will be printed in Terrestrial Magnetism.]

THE THEORY OF PERTURBATIONS AND LIE'S THEORY OF CONTACT TRANSFORMATIONS. By Dr. EDGAR ODELL LOVETT, Princeton, New Jersey.

[*Communicated by the Secretary.*]

IT is the object of the paper to present the theory of perturbations as a branch of Lie's theory of contact transformations. The investigation falls into the following divisions:

1° The Theory of Perturbations of Lagrange and Poisson;
2° The Theory of Hamilton and Jacobi;
3° Lie's Theory of Contact Transformations;
4° Schering's Development of the Hamilton Jacobi Theory;
5° Lie's Memoir on the Theory of Perturbations and the Theory of Contact Transformations.

The key to the connection between the theory of perturbations and that of contact transformations lies in the conception of a perturbation problem as a transformation problem of a system of canonical equations

$$dq_i = \frac{\delta F}{\delta p_i} dt, \quad p_i = -\frac{\delta F}{\delta q} dt$$

into a similar canonical system.

These canonical equations were first discovered by Lagrange for the case of a_i, β_i, where a_i and β_i are the initial values of p_i and q_i respectively, and F contains the q_i but not the p_i. They were extended by Hamilton to the case in which F contains both the p_i and the q_i; and finally by Jacobi to the case in which a_i and b_i are any system of conjugate elements. The first published demonstration of Jacobi's theorem was given by Desboves; demonstrations by Bour and Jacobi appeared later. Further it is to be remarked that the transformation defined by this theorem of Jacobi is not what Lie has defined as a contact transformation.

The equations of motion of a free system of particles under their mutual attractions and repulsions were expressed in this canonical form by Cauchy and Hamilton; by Binet for the case where the coördinates are connected by equations of condition; by Ostrogradsky for the case of a force-function containing the time explicitly; further, this canonical form was given to the general system arising from any problem in the calculus of variations by Ostrogradsky, and finally by Schering to the equations of motion when the forces depend not only on the position but also on the motion of the system. As particular cases, the formulæ for the perturbation of a body under the action of a central force, of a rotating solid, and of a projectile appear under the same canonical form.

Lie has shown that the transformation of any canonical system as considered by Hamilton and Jacobi, namely, that in which the function F of the system contains only the two sets of variables, into another canonical system is effected by a contact transformation. Also that the transformation of a definite canonical system in which the function F contains both sets of variables and the time, into another definite canonical system whose function Φ contains both sets of the new variables and the time, can be performed by a determinable contact transformation. The most general transformation which changes a definite canonical system into a similar system is, in general, not a contact transformation. Lie has shown how to find this general transformation The problem of finding the general

transformation that shall change every canonical system into every other canonical system, in the same general way that a transitive group of point transformations transforms the points of space, is yet to be solved.

ON RATIONAL RIGHT-ANGLED TRIANGLES.—I. By ARTEMAS MARTIN, LL.D., U. S. Coast and Geodetic Survey Office, Washington, D. C.

1. STATEMENT of Pythagorean proposition with brief bibliography and a proof of the proposition.

2. Algebraic proof, or solution of the equation

$$x^2 + y^2 = z^2$$

in general terms.

3. Properties of the sides, or their peculiar relations to each other.

4. Table of triangles.

[This paper will be printed in the Mathematical Magazine.]

SOME RESULTS IN INTEGRATION EXPRESSED BY THE ELLIPTIC INTEGRALS. By Prof. JAMES MCMAHON, Cornell University, Ithaca, N. Y.

THIS paper expresses in terms of the tabulated E and F functions a number of integrals many of which had apparently never been completely worked out.

[This paper will be printed in the Annals of Mathematics.]

MODIFICATION OF THE EULERIAN CYCLE DUE TO INEQUALITY OF EQUATORIAL MOMENTS OF INERTIA OF THE EARTH. By Prof. R. S. WOODWARD, Columbia University, New York, N. Y.

THIS paper shows how to express the effect of a small difference in the equatorial moments of inertia of the earth on the period of revolution of the instantaneous axis of rotation around the axis of figure. A remarkable value is obtained for the average angular velocity of that revolution; and a formula is deduced for the difference in the equatorial moments essential to explain the discrepancy between the observed and computed value of the Eulerian cycle.

[This paper will be printed in the Astronomical Journal.]

INTEGRATION OF THE EQUATIONS OF ROTATION OF A NON-RIGID MASS FOR THE CASE OF EQUAL PRINCIPAL MOMENTS OF INERTIA. By Prof. R. S. WOODWARD, Columbia University, New York.

THE title of this paper explains its contents sufficiently. The case considered is that of no applied forces, or that in which there is conservation of moment of momentum. The problem is of practical interest in its application to the question of variation of latitudes on the earth. Several new theorems with respect to the motions of the mass are derived.

[This paper will be printed in the Astronomical Journal.]

GENERAL THEOREMS CONCERNING A CERTAIN CLASS OF FUNCTIONS DEDUCED FROM THE PROPERTIES OF THE NEWTONIAN POTENTIAL FUNCTION. By Dr. J. W. GLOVER, Ann Arbor, Mich.

ON THE IMPORTANCE OF ADOPTING CERTAIN STANDARD SYSTEMS OF NOTATION AND COÖRDINATES IN MATHEMATICS AND PHYSICS. By Prof. FRANK H. BIGELOW, U. S. Weather Bureau, Washington, D. C.

THIS paper describes the present annoying state of the subject matter of notation and coördinate systems, advocates the adoption of certain standards, and discusses the possibility of obtaining a concensus on symbols and fundamental conventions.

[This paper will be printed in Monthly Weather Review.]

A REMARKABLE COMPLETE QUADRILATERAL AMONG THE PASCAL LINES OF AN INSCRIBED SIX-POINT OF A CONIC. By Prof. R. D. BOHANNAN, Ohio State University, Columbus, Ohio.

STEREOSCOPIC VIEWS OF SPHERICAL CATENARIES AND GYROSCOPIC CURVES. By Prof. A. G. GREENHILL, Royal Artillery College, Woolwich, England.

[For abstract see Section B.]

SECTION B.

PHYSICS.

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ADDRESS

BY

CARL BARUS,

VICE PRESIDENT, AND CHAIRMAN OF SECTION B.

LONG RANGE TEMPERATURE AND PRESSURE VARIABLES IN PHYSICS.

METHODS OF PYROMETRY.

THE endeavor to provide suitable apparatus for high temperature measurement is one of long standing. The student of the subject is fairly overwhelmed with the variety of devices which have been proposed. There are few phenomena in physics which have not in some way or other been impressed into pyrometric service, often indeed by methods of exquisite physical torture. I cannot, of course, even advert to many of these this afternoon, as my purpose will have to be restricted to such devices as have usefully survived. Thus a whole group of "intrinsic thermoscopes," as Lord Kelvin calls them, — apparatus in which some property of the substance is singled out for measurement — will be overlooked. Pyrometry will some day receive substantial aid from the phenomena of solid thermal expansion, dear to the hearts of old Wedgewood, of Professor Daniells, of the citoyen Guyton-Morveau and recently to Professors Nichols, Joly and others; but even the "meldometer," which has received Ramsay's encouragement, and recent heroic attempts to measure the expansion of platinum, have not yet entered the arena to stay.[1] The same may be said of vapor pressure, ebullition and certain dissociations,

[1] Noteworthy attempts to replace mercury by a liquid potassio-sodium alloy in glass thermometers are among the novelties.

of which the former is entirely too liberal in dispensing pressure, and the latter too negligent in readjusting it. Little has been done with heat conduction regarded as subservient to the measurement of high temperatures; little with color and the spectrum, even though Draper and Langley in this country and many others abroad have paid tribute; little with polarization. The wave length of sound has told Cagniard Latour and our own A. M. Mayer much about high temperature, but it did not tell them enough.

Throughout the history of pyrometry, *fusion* seems to have come forward for journeyman duty. What is more convenient than to find whether the degree of red heat is too low or too high from the fusion of prepared alloys. As far back as 1828 Prinsep, aware of the golden opportunity, with his golden air thermometer determined the melting point of some equally precious alloys of gold, silver and platinum, and determined them very well. Other alloys were afterwards substituted and graded mixtures made of quartz, chalk, kaolin and feldspar for the purpose. Efforts to obtain more accurate values are due to Becquerel, but the absolute values most widely used until quite recently, namely, the melting points of silver (958°), gold (1035°), copper (1054°), palladium (1500°), platinum (1775°), iridium (1950°), are due to the researches of Violle.

Interest in high temperature fusions has of recent date rather increased than abated. The demand for more accurate data has been met by the Reichsanstalt and we have now a set of values for silver, copper, gold, nickel, palladium and platinum in terms of the air thermometer standard of that institution. Data have also been supplied by Callendar. Among these values there is as yet considerable confusion and the end is not yet. Long ago I suspected that the Violle melting points were probably too low, whereas the assumed zinc boiling point is probably too high. This surmise has been partially borne out by the Reichsanstalt, though Le Chatelier even now prefers Violle's values.[1]

[1] The following table contains a brief summary:

	(Violle)	(Barus)	(Callendar)	(H. & W.)
Ag.	954°	986°–985°	982°	971°
Au.	1045°	1091°–1093°	1037°	1072°
Cu.	1054°	1096°–1097°		1082°
Ni.	——	1476°–1517°		1484°
Pd.	1500°	1585°–1643°		1587°
Pt.	1775°	1757°–1855°		1780°

Thermoscopes based on a *specific heat* have an advantage over fusion thermoscopes in not being discontinuous. They are quite as "intrinsic" and much less convenient in practice. Guyton-Morveau at the beginning of the century pointed out the pyrometer importance of specific heat and a host of observers followed him. But the critical discussion of the subject is due to Pouillet (1836), who determined the thermal capacity of platinum between 0 and 1200° absolutely, and found a value so nearly constant as to place this method of pyrometry in a very favorable light. Other observers followed with new data, and the bulk of our knowledge to-day is again due to Violle. Violle used Deville and Troost's exhaustion air thermometer and determined the law of variations of specific heat and temperature throughout a large pyrometric interval with a number of metals, silver, gold, copper, palladium, platinum, iridium, among them. It was by prolonging this law as far as fusion that the melting points of the metals, to which I have already alluded, were obtained. This verges on extrapolation, but it is not extrapolation gone mad.

The importance of calometric high temperature measurement has recently been accentuated in connection with the remarkable high temperature accomplishments of Moisson. Furnace temperatures in the case of such technological operations as are used in connection with iron, glass and porcelain manufacture, rarely exceed 1,400°; except perhaps in the Bessemer process where the temperatures are wont to exceed 1,600° and even reach 2,000°. In Moisson's furnace, which is essentially an electric arc enclosed by non-conducting lime, a totally new order of high temperatures are impressed. There was thus a call for at least an approximate measurement of their values which was answered by Violle, assuming that the specific heat of carbon above 1,000° approaches a limit of 0.5 calorie. The sufficiency of this hypothesis is not unchallenged, however; for instance, Le Chatelier finds that, up to 1,000°, the specific heat of carbon continually increases having no certain limit. Admitting Violle's results, Moisson's furnace temperatures exceed 2,000° even at 30 amperes 55 volts; at 360 amperes and 70 volts tin and zinc oxides melt and boil; they exceed 3,000° at 500 amperes and 70 volts, where lime melts, and often boils. Moisson, however, went as far as 1,000 amperes at 50 volts.

The striking novelty of Moisson's work is rather of chemical interest, and a large part of it is so fresh in our memory that in

view of Moisson's forthcoming book,[1] I need merely glance at it. A range of fusibilities, among which platinum lies lowest, while chromium, molybdenum, uranium, tungsten, vanadium, follow in order, and of ebullitions beginning with silica and zinc oxide, is rather breath-taking. Finally his structural investigations on the occurrence of carbon, and his long series of carbides, many of them commercially valuable, have staggered even the sensational press.

Leaving other intrinsic thermoscopes for the moment, I will ask your attention in this place to the development of the only fruitful method of absolute pyrometry which has yet been devised. I refer, of course, to the gas thermometer, or in other words, to the manometric methods of measuring the thermal expansion of gases. Efforts have indeed been made to use gaseous viscosity for absolute high temperature work. It has been definitely pointed out, inasmuch as viscosity in gases is independent of pressure, while both viscosity and volume increase with temperature, that the transpiration rates of gases through capillary tubes of platinum glazed externally would necessarily be an exceedingly sensitive criterion of the variation of high temperatures. The small volume of the transpiration pyrometer as compared with the clumsy fragile bulb and appurtenances of the air thermometer is further to the point. But modern kinetics has as yet failed to fathom the law of variation of viscosity with temperature, and even the researches of O. E. Meyer in this direction do not seem to have quite touched bottom. Gaseous transpiration pyrometry is thus still much in the air, surveying the horizon of a glorious future.

Returning from this digression to the air thermometer, we find the first thorough-going piece of high temperature work carried out by Prinsep (1829), by the aid of a reservoir of pure gold to which I have already alluded. Prinsep's manometer was filled with olive oil, and the volume issuing at constant pressures was found by the balance. In view of the pure olive oil, probably available in 1829, these experiments must have been comfortably appetizing and I dare say Prinsep's good humor in the matter may have contributed to the remarkable excellence of his results. Prinsep's researches were not superseded until Pouillet, in 1836, published his paper on pyrometry. Pouillet constructed an air thermometer bulb of platinum and was thus able to reach the farthest pyrometric north of the day and long after. His results are many sided; they con-

[1] Le Four électrique, par Henri Moisson; Paris, Steinheil.

tain the first definite data in radiation pyrometry and in calometric pyrometry. His constant pressure manometer, afterwards further perfected by Regnault, is the best apparatus for the purpose to-day. Pouillet did not suspect, indeed he remained quite unaware of, the permeability of platinum to furnace gases; perhaps for this and other reasons he failed to detect the thermo-electric anomalies in the platinum-iron couple which he so carefully calibrated.

It was thus a great step in advance when Deville and Troost long after replaced platinum by glazed porcelain, availing themselves (1857-60) of Dumas' famous vapor density method for measuring temperature. Unfortunately for the rapid progress of pyrometry, Deville and Troost used iodine vapor in their bulbs, a heavy gas indeed; but a gas, as was afterwards found, whose low temperature molecule dissociates at higher temperatures. Thus they unwittingly committed an even greater error than Pouillet in gliding over permeable platinum; and their data for the boiling points of zinc and of cadmium were about 100° too high. In fact these results were challenged not long after by Becquerel (1863) who had fallen heir to Pouillet's platinum air thermometer, had used it to calibrate a platinum-palladium thermo-couple of his own, and had found data for the boiling points of zinc and cadmium upwards of 110° degrees below those of Deville and Troost. I cannot here enter upon the discussion which thereafter arose between these active observers, further than to state that in the course of it both parties frequently repeated their measurements (Becquerel even substituting a porcelain bulb for Pouillet's thermometer) without removing the discrepancy between their values.

Later researches have decided in favor of Becquerel's results, and his original research, with its applications to fusion, to radiation, to thermo-electrics, etc., is one of the noteworthy accomplishments in the history of pyrometry. Nevertheless it must not be forgotten that, to the investigation of Deville and Troost, our knowledge of the perviousness of iron, platinum and other metals to gases is due. We are also indebted to Deville for the great discovery of dissociation, the very phenomenon which he was here so loth to acknowledge. This is the case of a man stumbling in his own footprints. Victor Meyer was, I believe, the first to point out the probable dissociability of the iodine molecule, suggesting a fruitful subject of research which has since been extended to many other molecules.

In 1863, Deville and Troost began a new series of high temper-

ature researches, the feature of which is the perfected form of porcelain bulb. This was a hollow sphere and long capillary stem adapted for use with Regnault's standard air thermometer. Great difficulties were encountered in the endeavor to glaze the bulbs within. They were finally overcome by making bulb and stem separately and then soldering them together with feldspar before the oxy-hydrogen blow pipe. Elaborate measurements on the thermal expansion of Bayeux porcelain accompany these researches which, undertaken together with M. Gosse of the Bayeux works, occupied them intermittently for about seven years. A full summary of their data did not appear, however, until 1880, when, together with a new vacuum method of high temperature air thermometry, they communicated the results of twenty-seven measurements on the boiling point of zinc. Their new results are in good accord with the data of Becquerel already cited and the more recent results of Violle and others for the same landmark in the region of high temperatures. Measurements between 0° and 1500° had thus reached a degree of precision of about 15° in 1000°.

The further development of pyrometry took a somewhat different direction. Regnault (1861) had already made use of a displacement method, in which the measuring gas is removed bodily into the measuring apparatus by an absorbable gas. But the method was independently revived by Professor Crafts of the Boston Institute of Technology. These methods are not of especial excellence below 1500°; but above that temperature when most solids tend to become viscous, their importance increases (as Crafts duly pointed out) in proportion to the rapidity with which the measuring operations can be completed. One or two minutes may suffice and different gases may be tested consecutively. It is in this way that Victor Meyer and his pupils, after demonstrating the dissociation of iodine and chlorine molecules, succeeded in penetrating quantitatively to very much less accessible heights of temperature. A particular desideratum was a rigid test as to the stability of the molecule of the standard measuring gases (oxygen, hydrogen, nitrogen). The results were favorable inasmuch as for these and for many gases like CO_2, SO_2, HCl, Hg, etc., the expansions obtained were linear functions of each other.

In their final work, temperatures as high as 1700° were reached, the air thermometer for this purpose being tubular in form, consisting of very refractory fire clay with an interior and exterior lining of platinum and with two end tubulures of platinum for in-

flux and efflux of gases. Among many results of great chemical interest their researches showed that metallic vapors, phosphorus, sulphur, etc., at high temperatures tend to pass into the monatomic, or the diatomic molecular structure.

Sometime after (1887) a series of experiments furthering the line of research of Deville and Troost were made with a geologic aim in view in the Laboratory of the U. S. Geological Survey. Finally, porcelain air thermometry was taken up with great vigor by the Reichsanstalt. These results due to Holborn and Wien are now almost exclusively quoted and carry the stamp of the great Institution from which they emanated. They have been wisely made commercially available by the deposition with Heräus in Hanau (Germany) of a platinum rhodium alloy definitely calibrated for a temperature range of 1400°.

Apart from this, these researches contain no essential novelty except perhaps a more detailed attempt to investigate the stem error of the thermometer bulb; their procedure otherwise is identical with the method developed in this country. I am not therefore inclined to yield to it the unhesitating deference which has become customary. There can be no doubt, in view of the splendid facilities due to the coöperation of the Royal Prussian Porcelain works—facilities which those who have been baffled by porcelain technology, or have had to coax unwilling manufacturers into reluctant compliance, will appreciate—that the data of the Reichsanstalt will eventually be standard. For the present, however, I should be more impressed by some sterling novelty either in the direction of a larger range of measurement, or of method. Conceding that an accuracy of 5° at 1000° has been reached, all results above 1500° remain none the less subject to increasingly hazardous surmise.

A beautiful method of absolute thermometry, albeit as yet only partially developed, is due to Töpler. In this the densities of communicating columns of gas are compared very much as in Dulong and Arago's classical method for liquids, by the gravitation pressures which correspond to these unequally hot columns. To accomplish such extremely fine pressure measurement, Töpler invented the "Druck libelle" an inversion as it were of the common level, in which therefore the motion of the bubble (or of a thread of liquid) indicates a change of pressure conditioned by the invariable horizontality of the instrument.

The development of the practical forms of continuous intrinsic

thermoscopes (the *radiation*, the *thermo-electric*, and the electric *resistance* methods) went more or less hand in hand with the development of the air-thermometer although the latter is decidedly the more recent. Aside from pioneering experiments of Müller (1858) and others, the well known Siemens resistance pyrometer (1871) was the first instrument in the field. It was based upon data obtained from platinum, copper and iron by the calometric method of calibration. This instrument has been remarkably perfected by Callendar and Griffiths, using specially pure platinum calibrated by comparison with the air-thermometer as far as about 600°. Notwithstanding these improvements the resistance pyrometer is inferior in my judgment to the thermo-electric pyrometer from the greater bulk and fragility of the exposed parts and the tendency of platinum to disgregate or waste itself gradually at high temperatures. Its upper limit of temperature measurement is thus limited; for even if the difficulty of selecting suitable terminals for the coil is set aside, the difficulty of finding an insulator at very high temperatures would remain. According to Holborn and Wien, resistance is seriously subject to the influence of furnace gases and permanence of the low temperature constants does not imply a like permanence of the high temperature constants of the metal.

Radiation pyrometry, curiously enough, is the most venerable method within the whole scope of the subject. It was introduced by Newton (1701) in his *scala graduum caloris* in connection with his well-known law of cooling. Not to mention minor workers, it was successively attacked and revived in most of the noteworthy high temperature investigations. Pouillet and Draper have studied it; Becquerel, Crova, Violle, Le Chatelier, Langley, Nichols, Paschen and others have advanced it. It remains to-day the most promising, as well as puzzlingly fascinating subject for pyrometric research. One need merely advert to its broad scope in relation to the temperature of the heavenly bodies to acknowledge this. Here I can only allude to Becquerel's principle that the radiation of opaque bodies is spectrometrically alike at the same temperature, a result which has Crova's more recent assent; to Violle's photometric measurements of the total emission of platinum; to the more recent work in the same direction of Violle and Le Chatelier in which consistent results were obtained for oxide of iron and platinum as far as 1500° to 1700°; to Stefan's law as proved by Boltzmann and the variety of discussion it has elicited; to

H. F. Weber's collateral equation; to the Johns Hopkins measurements, etc. Another school of observers, including Langley, Paschen and others, has undertaken the promising but much more laborious method of bolometric measurement of the distribution of spectrum energy in its relation to temperature. Without doubt, however, the whole subject is yet *in primis rudimentis;* the results are confessedly "intrinsic." Indeed vagueness in the nature of the radiating source lowers with sufficiently threatening aspect to chill the fondest hopes. When one is told by Violle working on Mont Blanc that the temperature of the sun is 2500°; thereupon by Rossetti that it is 9965°, by Le Chatelier that it is at least 7600°, by Paschen that it is below 5000°, by Wilson and Gray that it is 8000°, etc., one wisely concludes that more may yet be learned about it. Our sympathies naturally go with those who, like Lummer and Wien and the Johns Hopkins people, are beginning fundamentally with the search for an absolutely black body. Less superstructure and more sub-cellar is perhaps the watchword in radiation pyrometry.

Turning now to the last and most important of the methods of practical pyrometry, we find a curiously meandering evolution apparent. I have already indicated that Pouillet (1836) was the first to complete a legitimate piece of calibration work. Pouillet might have condemned the method, but for some reason Tait's thermo-electric anomalies of red-hot iron were not detected. Regnault (1847), who was the next to take up the subject as it happened with the same couple, made this condemnation sweeping enough. It was not the real perversity of the platinum iron couple which provoked Regnault, for of this neither he nor Pouillet became aware. Regnault's objection (as we should put it to-day) lay in the fact that the thermo-couple obeyed Ohm's law, which in that early day lay somewhat beyond the great physicist's range of interest. Fortunately, but none the less long after, Becquerel followed with his palladium and divers platinum couples, carefully calibrated and efficiently used. What these platinum couples were is not stated. They cannot have been very sensitive or they would have been preferred to the palladium-platinum couple. Indeed the metallurgy of platinum alloys did not reach a degree of refinement until Deville and Debray (1875) overhauled the chemical separation of platinum metals with particular reference both to iridium and to rhodium. Recently Mylius and Förster at the Reichsanstalt further

contributed to platinum metallurgy. But in view of the toils in which the whole subject of high temperature measurement languished in Becquerel's day, his results were not sufficient to remove the discredit which Regnault had thrown upon thermo-electric pyrometry. And so it happened that the return to the method in recent date was of the nature of a resuscitation.

It is amusing to note, as we pass on, the pranks of custom as it bore down upon pyrometry. Following Deville and Troost every worker (I might mention at least five) felt in duty bound to redetermine the boiling point of zinc,—rather a difficult feat in its way. Thus we find boiling zinc inseparably associated with the destiny of the calibrated thermo-couple. Le Chatelier broke this law of fateful sequence by ignoring the need of calibration at the outset, and then using the couple so dignified to determine the melting points of silver, gold, palladium and platinum. But these are Violle's melting points. Hence the pyrometric feature of Le Chatelier's platinum-rhodium couple was in its inception due to Violle.

Meanwhile, accompanying the geologic inquiries of Clarence King, an extensive series of pyrometric investigations which had been in progress in this country since 1882 were completed (1887). These contained a full examination of divers efficient methods of pyrometry and a study of the porcelain air-thermometer with particular reference to the calibration of thermo-couples. In the course of this work the admirable pyrometric qualities of the platinum iridium alloy were exhibited by detailed and direct comparisons with the air thermometer. It was shown that the calibration could be made permanent by referring the thermo-electromotive forces to a Clark's cell; that the character of their variation with temperature is uniformly regular and that the thermal sensitiveness of the couples increases as the higher degrees of red and white heat are approached. Finally it was pointed out that couples destroyed by silicate corrosion or in similar ways could be restored by fusing over again on the lime hearth with merely negligible changes of constants. Elsewhere, Le Chatelier's clever combination of the platinum rhodium couple with the D'Arsonval galvanometer, then a comparatively new instrument in the laboratory, secured immediate favor. Professor Roberts–Austin, ever on the watch to waft good things across the channel from Gaul into Albion, hailed the new comer with no uncertain sound. Sometime

after, the platinum-rhodium couple entered Germany and was there definitely calibrated (1892) for the first time as already stated, at the Reichsanstalt.

Of the three available couples, palladium, platinum-rhodium and platinum-iridium, the former is excluded from competition by reason of its low fusibility. Between platinum-iridium and platinum-rhodium, the latter has been more extensively advertised but is otherwise inferior to the older platinum-iridium alloy. In other words platinum-iridium, when suitably alloyed, can be made more sensitive than platinum-rhodium in the ratio 100 to 76. Beyond this the alloys are much alike; both are tenacious, resilient, refractory metals and their thermo-electric forces under like conditions of temperature show a constant ratio even at extreme white heats. The thermo-electric activity of these two alloys is exceedingly remarkable. Among over fifty different platinum alloys examined no similarly sensitive combinations were found. Moreover, whereas platinum alloys of extremely large electrical resistance are not unusual, such metals are not apt to be distinguished thermo-electrically.

To conclude: the small dimensions of the sensitive point of the thermo-couple, the independence of the intermediate temperatures between the junctions (apart from small corrections due to the Thomson effect) and therefore the removal of the terminal difficulty, the high upper limit of the measurable temperatures, the permanence of its constants in relation to Clark's cell in the lapse of time, the instantaneity of its indications, the easy reproduction of destroyed couples, their relative insensitiveness to furnace gases, the regular and simple character of the temperature function, the sustained sensitiveness throughout all temperature ranges even quite into the fusion of platinum — all these facts are a sufficient if not overwhelming recommendation of the method.

In speaking of long range temperature variables, one is hardly permitted to overlook the remarkable work which has recently been done in the direction of low temperature: but with these subjects I am less familiar and can therefore only refer to in passing. The progress made in the subject is sufficiently evidenced by the growth of large low temperature laboratories throughout the world, laboratories which undertake "the cold storage" of "cold storage" as it were, like those of Pictet in Berlin and Paris, of Dewar in London, of Kamerlingh Onnes in Leyden, of Olezewski in Krakau, and

others. Dewar and Fleming have added to our knowledge of the probable constitution of bodies at the absolute temperature. Olezewski has found the critical temperature of hydrogen at — 230° and its atmospheric boiling point at — 243°. Dewar and Moisson have liquefied fluorine. There is much here which I must reluctantly forego. The hydrogen thermometer, the platinum balance (Callendar) and the thermo-couple are again doing excellent work in thermometry.

APPLICATIONS OF PYROMETRY.

Turning now to the applications of recent pyrometry, we meet first many series of valuable data on melting points and similarly valuable data on the dissociation temperatures of chemical compounds. To these I merely refer not being qualified to enter into chemical interpretations. High temperature boiling points have also been treated and I will especially consider the case of the variation of metallic ebullition with pressure. The relation of vapor pressure to temperature has thus far defied the counsels of the wise even though such men as Bertrand and Dupré have given the matter close scrutiny. One would suspect the simplest relation to hold for metallic boiling points, and investigations have therefore been undertaken in which the temperature of ebullition of Hg, Cd, Zn, Bi, were studied for pressures decreasing from one atmosphere down indefinitely.

The results so obtained show an effect of pressure regularly more marked as the normal boiling point is higher so that the attempt to express the phenomenon for all these bodies by a common equation is roughly successful. By far the most rapid reduction of boiling point occurs when the pressure decreases from $\frac{1}{10}$ atmosphere indefinitely. For the case in which the normal boiling point is to be predicted from a low pressure value in case of a metal which, like bismuth, boils with great difficulty, very high exhaustion is essential.

Igneous fusion, by which I mean the fusion of rock-forming magmas, is particularly interesting in its relation to pressure. This has been again recently pointed out by Clarence King in his discussion of the age of the earth. If the earth is solid within, as is now generally admitted, such solidity can only result from superincumbent pressure withholding fusion. To study the relation of melting point to pressure directly is out of the question when white

heat is the condition of fusion. In this respect the laboratory in the interior of the sun or even of the planets has some salient advantages; but we cannot comfortably put such a laboratory under strict surveillance of protoplasm.

Fortunately the Clapeyron equation, successively improved by James Thomson and by Clausius, is here usefully available. To measure the melting point, the difference of specific volumes of the solid and the liquid body and the latent heat of fusion at this temperature, with the aid of Joule's equivalent is to measure also the relation of melting point to pressure implicitly. Based on the first and second laws of thermodynamics, this equation is generally true, no matter what specific properties may characterize the body. The process has thus far been completely pushed through for diabase only. Thermal change of volume may be measured by enclosing the rock in a platinum tube of known expansion, and the contraction of the contents from liquid to solid found by an electric micrometer probe reaching within the tube. Given a furnace fully under control, then as experiment has shown, the cooling can be made to take place so slowly that platinum remains rigid relatively to its charge of red-hot magma, and under these conditions the contraction can actually be followed into the solid state. At the same time the temperature at which marked change of volume occurs is the melting point. Apart from difficulties of manipulation the latent heat may be found from measurement of thermal capacity on either side of the temperature of fusion, by a modification of known methods.

The rate at which fusion is retarded by pressure computed from these data in the manner specified showed an increase of the melting point of a silicate of about 0.025°C. per superincumbent atmosphere. But this datum falls within the margin (.02 . . . .04) of corresponding data much more easily and directly derived for organic bodies. One may therefore argue that if the melting point pressure rate is so nearly constant on passing from the class of silicious to the thoroughly different and much more compressible class of organic bodies, the rate would probably be more nearly constant in the same body (silicious or organic) changed only as to temperature and pressure. This surmise was verified for naphthalene within an interval of 2000 atmospheres.

The endeavor to interpret the change during fusion of the volume of the chemical elements in terms of the periodic system has been

begun with much success by Max Töpler for low temperatures. It would be of great interest to complete this diagram for high temperatures in view of the specifically molecular character of the fusion phenomenon, by repeating such experiments as have just been described for rock magmas.

The heat conduction of rocks has been investigated in many cases for temperatures lying below red heat. Among recent observers we need only instance the extensive investigations of Ayrton and Perry. No trustworthy experiments, however, have yet been carried into the region of essentially high temperature; and yet what is chiefly of interest in the geological applications of such experiments is the change of conduction which accompanies changes of physical state, whether induced by pressure or by temperature.

Experiments in heat conduction are difficult under any circumstances. They become insuperably so when conduction at white heat is to be studied under pressure, and that is what the geological conditions actually imply. Some notion of a body respectively solid and liquid at a given temperature may be obtained by observing the behavior of bodies which are capable of being undercooled. A number of such bodies are known, thymol being a conspicuous example. Experiments with this body were made by measuring the volume expansion, specific heat, and heat conduction in parallel series both for the solid and liquid state at like temperatures. They showed, for instance, that the increment of absolute heat conduction, encountered in passing isothermally from the solid to the liquid state, when referred to solid conductivity is about 13%, and when referred to a liquid conductivity is about 15%. Similarly the change of thermometric conductivity under like conditions is an increment of 36% and 56% respectively. Now, since in most questions relating to thermal flow, thermometric conduction enters exclusively, the importance of this large coëfficient is obvious whenever a body passes from the solid to the liquid state.

Solid conduction is thus 40 or 50% in excess of liquid conductivity for the same body at the same temperature and pressure. It is reasonable to infer that a corresponding decrement of conduction will accompany any rise of temperature of a solid body. Measurements which have somewhat recently been made for relatively small intervals at Zurich, at Glasgow and at Harvard upon typical rocks, all bear out this surmise. The immediate incentive to these experiments was a strong paper by Professor Perry in which Lord

Kelvin's estimate of the age of the earth is shown to be insufficient for an earth in which the interior conductivity is systematically greater than the surface conductivity. Indeed he deduces the percentage increment of the square root of the age of a Perry earth over that of a Kelvin earth to be one-fifth of the percentage decrement of conduction for each 100° C. So far as the effect of terrestrial temperature alone is concerned the measurements just mentioned show that Perry's correction is negative or that Perry's earth would be less long lived than the 100×10^6 limit of years set by Lord Kelvin.[1]

To estimate the effect on heat conduction of the increase of pressure which accompanies the increase of temperature with the depth below the surface is a much more serious matter. In the laboratory, pressure experiments are limited to 3,000 or 4,000 atmospheres; compared with earth pressures these scarcely amount to a scratch on the surface; yet even for this limit the determination of heat conduction at high temperatures is out of the question. A tentative method of arriving at a conclusion is given by Clarence King in his discussion of the age of the earth, the consequences of which have been quite overlooked. What King endeavored to accentuate long before Perry's contribution to the subject was precisely the fact we *cannot* assume greater conductivity for the interior than for the surface. Since heat conduction decreases isothermally from solid to liquid, it was assumed that in one and the same substance, the viscosity could be taken as an index of the thermal conduction. Therefore if temperature and pressure were made to vary in such a way (both increasing), as to leave viscosity constant, it was inferred that heat conduction would also remain constant. Now the isometrics or lines of constant viscosity of a viscous body for variable pressure and temperature are much more easily found than the isometrics of conduction. In fact, it has been shown that a burden of at least 200 atmospheres would have to be brought to bear in order to wipe out the decreased viscosity due to the rise of a single degree (Centigrade) of temperature. The depth at which this ratio is reached, as King points out, for a given surface gradient of temperature and depth, depends on the initial excess of the temperature of the earth considered, and on the age of the temperature distribution resulting. But no

[1] The text of Kelvin's recent address at the Victoria Institute, in which an age of thirty million years is maintained, has not yet reached me.

matter whether the Kelvin earth with an initial excess of 3,900° and an age of 100×10^6 years, or whether King's solid earth with an initial temperature of fused platinum and 25×10^6 years of life be taken,—in all cases the temperature effect predominates throughout those depths within which change of temperature with depth is the marked feature of the temperature distribution. In other words, if for example we consider the Kelvin earth, the strata above .035 of earth radius will be strata of smaller conduction than the surface strata. From the surface downward as far as .035 radius, conduction will decrease to a minimum. Below this, conduction will increase again due to preponderating pressure, finally to exceed the surface value. But the computed temperature distribution of Kelvin's earth is such that at .035 radius the initial temperature excess of 3,900° has been reached to within one or two per cent. Below this in depth, Perry's correction would begin to apply, but the further changes of temperature are so nearly negligible that the consideration of conduction is superfluous. From this point of view, therefore, the staggering force of Perry's clever argument is removed. Of course, I am fully aware that an argument from the supposed parallelism of physical properties of a given body (in the present case heat conduction and viscosity) is not inevitably convincing; but in physical geology, for some time to come I dare say, the question will be not one of rigorous proof but rather one of forming a rational opinion.

In passing I will indicate the importance of an increased knowledge of the isometrics of liquid and solid matter, relations which have thus far been found simpler in character than other thermodynamic properties, as I shall again point out in the course of the address.

I want finally to add a few words on the electro-chemistry of magmas. The physical chemistry of molten rock has already been somewhat extensively considered, but I am hardly competent to review it. In this country, Joseph Iddings and, more recently, George F. Becker have discussed the natural differentiation of magmas from different points of view. Here I will merely include certain pyrometric experiments on the electric conduction of fused glasses which seem to give promise of throwing light on the chemical constitution of complex silicates and to be suggestive in other ways. In such measurements, if the magma is made to pass from the solid to the liquid state, the observed electric conduction contains

no evidence either of a melting point or of polymerism. The law of thermal variation is easily derived and it agrees closely with the corresponding behavior of a zinc sulphate solution for instance, observed through a range of temperature. In a general way, electric resistance decreases in geometric progression when temperature increases in arithmetrical progression. Considered relatively to the composition of the magmas, electric conduction shows a marked and regular increase with the degree of acidity of the magmas. The acid magmas are better conductors than the basic magmas at the same temperature. Curiously enough conduction thus runs in an opposite direction to fusibility. However viscous a magma may be, therefore, and however cogent the arguments such as those launched by Becker against the differentiating importance of ordinary diffusion may prove, it is fair to conclude that a thorough change of chemical structure through *ionic* diffusion, whether directed by an electric field or otherwise, must be an easy possibility for a sufficiently hot, but otherwise solid magma. The results point specifically to the desirability of repeating Hittorf's brilliant experiments on the migration of the ions for a silicious medium. This ought not to be difficult, seeing that such a menstruum need not even be liquid to be compatible with a high order of electric conduction.

Further consideration of the subject shows the probable passage of conduction through a maximum when acidity is continually increased. Finally quartz appears like an insulator in the same rôle as water in ordinary aqueous solutions. In all these cases I wish to keep in mind the results of Alexeëff and their recent repetition for metallic alloys, together with the interpretation of these results due to Masson. In a crust subject to variable magnetism, traversed by earth currents, sustained by semi-metallic carbides of the Mendeleëff-Moisson type, containing piezo-electric and thermo-electric sources, who can say that electric fields are absent. Again the character of the changes contemplated in Gibbs' famous "phase rule" as interpreted by Le Chatelier, would here be ionic rather than molecular.

A question of somewhat allied interest is the action of hot water under pressure on rock-forming silicates. Investigations of this kind have been described in the well-known and fascinating book of Daubrée. Daubrée's work, however, is qualitative in character like that of many others in the same line, and the furtherance

of the subject is to be looked for in the quantitative direction. Some time ago, Becker suggested experiments on a huge mass of granulated rock under the action of steam at exceptionally constant temperature. But no thermal effect of the action of water could be detected. True, the boiling point of water is a temperature relatively low for the purpose; yet similar experiments made with liquid water at over 200° under pressure were equally negative as to results. Experiments of this kind are not very conclusive. The insufficient sensitiveness of the measuring apparatus, the rate at which heat is carried off compared with the rate of generation, and other obscure causes mar the results. The question, may, however, be approached in a somewhat different way: If water is heated under pressure in glass tubes, the volume of water contained decreases as the square, whereas the chemically active area, *i. e.*, the inside surface of the tube, decreases as the first power of the diameter. Hence, in proportion as the tube is more capillary, the action of water on the glass will produce accentuated volume effects. Thus it was shown that the behavior of hot water is profoundly modified by its continued action on glass, inasmuch as its compressibility increases at a very rapid rate with the time of action even at 180°, until, with the approach of solidification, the observed compressibility is fully three times its isothermal value at the inception of the experiment. Even more striking is the simultaneous and continual decrease of the length of the column of water. Clearly, therefore, the confined volumes of glass and included water must undergo contraction at 180° in forming an eventually solid aqueous silicate, while increasing compressibility is due to the increasing quantity of silicate dissolved. Now, in nearly all cases the effect of solution is a decrease of compressibility. Hence the increased compressibility observed is to be referred to a precipitation of the dissolved silicate, in response to the action of pressure, a result borne out by the appearance of the tube and by varied correlative experiments. It is, however, the volume contraction which is particularly interesting, because of its far-reaching geological application. In the first place, the measurements show that about .025 cubic cm. of liquid water is absorbed per square centimeter of glass surface at 180° C. per hour.[1] The effect of this absorption is a contraction of bulk amounting to 18% per hour. So large and rapid a contraction

[1] This is an initial rate of about 180 kilograms per square meter per year.

is presumably accompanied by the evolution of heat. Hence, under conditions given within the first five miles of the earth's crust, *i. e.*, if water at a temperature above 200° and under sufficient pressure to keep it liquid be so circumstanced that the heat produced cannot easily escape, the arrangement in question is virtually a furnace whose efficiency accelerates with rise of temperature.

PIEZOMETRY.

It is not feasible to make much progress in pyrometry without feeling the need of a corresponding development in high pressure measurement. This has already appeared in the preceding parts of my address. It will not be expedient to look into the history of the subject so comprehensively as I did in the case of pyrometry partly because the literature is more diffuse and partly because the real development of piezometry is of recent date and virtually begins with pressures of the order of several thousand atmospheres. So understood, although we gladly pay homage to Oersted, to Regnault, to Grassi and many others, our historical summary may be abridged.

As is often the case in physics, the great advances in the subject are associated with the name of one man; for though many able investigators have contributed effectively to the progress of piezometry, the overshadowing importance of the results of Amagat have superseded all researches coextensive with his own. For over twenty years Amagat has been laboring on this definitely circumscribed subject. Year after year his prolific experimental ingenuity has put forth results, each of which in its turn constituted the highest attainment in accuracy and the greatest breadth of scope which high pressure measurement had reached at the time. It is impossible to give any adequate view of this sustained labor in an address. The subject is highly specialized and demands special treatment; but we owe to Amagat the bulk of our knowledge of the properties of a gas regarded not as an ideal fluid, but as a physical body; some of the most far-reaching results in thermo-dynamics of liquids and some of the best data in the elastics of solids.

Amagat investigated gases within an interval of pressure which at times reached 4000 atmospheres with a view to interpreting their divergence from the laws of ideal gaseity. Indeed we may note in

passing that just as the advanced astronomy of the day is being enriched with unexpected discoveries from a discussion of mere errors of observation, so refined physical measurement gleans new harvests in carefully tracing out the all but rigorous sufficiency of established laws. The product of pressure and volume, nearly constant in the ordinary isothermal behavior of gas, shows, under higher pressures, a well marked passage through a minimum in the case of all gases except hydrogen. Hence below a certain definite pressure, varying with the character of the body (say 40 atm.), gases are more compressible than Boyle's law asserts, and above this pressure they are continually less compressible and begin to resemble hydrogen in this respect. The sharpness of the minimum diminishes as temperature increases and probably ulti nately vanishes. Cailletet, it is true, had undertaken a study of the same subject simultaneously, but his results were not in the same degree correct. Again, the coëfficient of expansion of gases considered in its isopiestic behavior for temperatures not too far above the critical point, increases with pressure to a maximum. which seems to occur at the same pressure for which the volume-pressure product is a minimum. This thermal maximum also decreases with temperature and finally vanishes. To specify the conditions further than this would be to exceed the limits beyond which verbal statement ceases to be lucid. The value of Amagat's work, however is not merely the formulation of such general laws for gases as a whole, but rather the investigation of sharp and specific results for each gas individually. Thus if one uses these data for a given gas to compute the constants in Van der Waal's law, one is actually able to predict remote critical conditions of the gas in question with a fair order of accuracy.

Whenever pressure measurements are to be made through such large intervals as are here in question, the elastic constants of the apparatus become of increasing moment. Amagat, however, treated these incidental measurements as of like importance with the rest of his labors. The starting point of his investigation into high pressures was the open mercury manometer first erected along a staircase near Lyons, finally in the shaft of the St. Etienne mine. about 380 meters deep. This apparatus was used for graduating the closed manometer. preferably containing nitrogen. In later experiments for excessively high pressures, the closed manometer was replaced by the "manomètre à pistons libres," a sort of

inverted Bramah press, in which the small pressures of an open mercury manometer acting on a large piston compensate the relatively large pressures of the piezometer acting on a small piston The ingenious feature of Amagat's apparatus is the rotation of both pistons just before measurement, a device by which friction is rendered harmless. Equipped with this instrument direct determination of the bulk modulus for glass and metals was actually feasible. In the case of glass no serious variation of the compressibility could be ascertained within 2000 atmospheres and even 200°, an observation of great value in practical research. Poisson's ratio was similarly determined and the data used in computing Young's modulus. But the most important result of these researches, a result to which Professor Tait also contributed, is the datum found for the absolute compressibility of mercury. This will enable all future observers in piezometry to standardize their apparatus with ease and nicety.

Time prevents me from dwelling upon the remaining investigations of Amagat in a measure commensurate with their value. These contain a counterpart for the liquid state of the results already announced for gases. The change of volume throughout enormous pressures and about 200° of temperature is considered in detail for a number of important liquids. Only in one case, and that the rather remarkable one of carbon tetrachloride, are evidences of solidification encountered, and the conditions determined. Amagat believes the absence of solidification to be due to the occurrence of the lower critical temperature below the isothermal of compression. In my own judgment, however, the pressures necessary to reach this lower critical point will be enormous even in units of 1000 atmospheres, for which reason it is not in any case liable to be an easy conquest.

Special mention finally is due to the thermal position of the maximum density of water, which Tait had already studied. Amagat shows definitely that the temperature of maximum density moves towards the freezing point with increasing pressure so that at high pressures, as well as at high temperatures, the behavior of water loses its anomalous character. In general, compressibility and expansion decrease with pressure for all normal liquids, and expansion increases rapidly with temperature. Other anomalous properties of water have been investigated among which the diminished viscosity of water under pressure at ordinary temperature studied by Röntgen, Cohen and others, may be stated.

After this cursory and wholly inadequate mention of the work of Amagat and the physicists who, like Tait, Cailletet and others, have been engaged in closely allied researches, it will repay us to look at some of the other as yet less splendidly developed contributions to piezometry. At the outset it is well to make mention of the forms of pressure gauges which have come into use. As far as 1000 atmospheres, the Bourdon gauge, if well constructed, does good service, though in a somewhat rough way. The corrected nitrogen closed manometer is more accurate for a smaller range. A theoretically simpler pressure gauge was proposed by Tait and Cailletet. In this case a straight cylindrical elastic tube under internal or external pressure is substituted for the Bourdon tube and the expansion or compression is directly measured. With due precautions against changes of temperature and the choice of a solid of constant bulk modulus and rigidity. this gauge can be used as far as about 2000 atmospheres with convenience.

Above 2000 atmospheres, Amagat's Bramah press manometer already mentioned is the only reliable gauge, and though somewhat cumbersome has the advantage of giving absolute results. However, a gauge based on the change of electric resistance of mercury with pressure, a constant now fairly well known from Palmer's measurements, will in my judgment do good service for pressures which exceed even the range of the manometer. With regard to methods for producing high pressures, the force pump, with a small steel plunger and the screw advancing bodily into a closed barrel filled with a liquid, have not yet been superseded. The efficiency of such apparatus depends essentially on the means used for obviating leakage. These must of course be very perfect.

Amagat's work with liquids was extended chiefly in the direction of high pressures. Experiments have since been made by others throughout higher temperatures (310°) and of course a smaller range of pressures (500 atm.). Leaving out the less perspicuous results, I will here merely allude to the probable existence of a remarkable law which these researches have developed. Dupré (1869) and afterwards Lévy (1878), reasoning from thermo-dynamic premises, were the first to suspect that the isometrics or lines of equal volume of liquids are straight. In other words, if there is to be no change of volume then pressure increments must vary proportionately to the temperature increments ($p = a\,\theta - b$), a result which implies that the internal pressure of a body kept at constant volume is proportional to its temperature. Lévy's deduction

was however declared to be theoretically unwarrantable by Clausius, Boltzmann and others. Sometime after, the same law reappeared in experimental form in a series of brilliant researches on critical temperatures due to Ramsay and Young. Fitzgerald, reasoning from Ramsay and Young's results, then proved that for such liquids as possessed straight isometrics, specific heat is a temperature function only and energy and entropy are each expressible as the sum of a mere temperature function and a mere volume function. This is curiously like the position from which Dupré and Lévy started. Ramsay and Young's work, however, applied specifically to vapors, and for high temperatures (200°) their pressures did not exceed 60 atmospheres. The law has since been tested for liquids as far as 1500 atmospheres and over 200° conjointly, and found in reasonable accordance with experiment. Hence we infer that if the thermodynamic change of a body is such that volume remains constant, pressure and temperature will vary linearly with each other, the increments being about 0. 1° C. per atmosphere. Now, although any law relating to the liquid state would be welcome, these volume isometrics are particularly so. In the geology of the earth's crust, for instance, they would in a great measure determine the conditions of possible convection; and it is curious to note that from the known values of bulk modulus and of the expansion of solid glass, the isometrics would here again be given by corresponding increments of about .1° per atmosphere. For solid metals the isometrics are of a different order.

Another line of research for liquids to which I attach supreme importance has only just been begun: I refer to the systematic study of the *entropy* of liquids. Among the first results on the heat produced in suddenly compressing a liquid are those of Tait. They are of too limited a range, however, and not in good accord with the more recent and extended data of Galopin. Generally speaking, the change of temperature produced per atmosphere of compression increases with temperature in a marked degree, as one may infer from Kelvin's equation. For organic bodies this increment at ordinary temperatures is of the order of $\frac{1}{50}° = .020°$ per atmosphere. In case of liquid metals the order of values is decidedly different, being about $\frac{1}{10}$ this value, recalling correspondingly divergent results observed for the isometrics of volume. Quite recently (1896), the same subject has been taken up by Tammann (to whom we also owe results for the correlative compressibility)

particularly for solutions and with reference to the theory of solutions. Tammann's data are of the order .001° per atmosphere at 0°, and in better keeping with the thermo-dynamics of the subject than the earlier experiments. Much more, however, must be done before anything like a degree of critical accuracy is approached or a broad survey taken. Pressure intervals are to be chosen wider and the temperature measurement given with greater certainty.

Finally, I wish to touch upon the relations of melting point and pressure in their more recent development. Obviously the classical work of Andrews on the continuous passage of a liquid into the gaseous state will find some counterpart in the manner in which the analogous passage from the solid into the liquid state takes place. The character of these phenomena may be shown from direct observations of melting point and pressure as was done by the earlier observers. Full knowledge, however, can be obtained only by mapping out the isothermals throughout the region of fusion very similarly to the method pursued by Andrews himself for vaporization. This has thus far been attempted for a single body only, naphthalene, within 130° and 2,000 atmospheres. Six isotherms (63°, 83°, 90°, 100°, 117°, 130°) were traced within these intervals along each of which excepting the first, the body passed from the liquid to the solid state under the influence of pressure only. An exhibit of these data shows strikingly that in all cases the change of physical state takes place in accordance with a cyclic law; *i. e.*, a larger pressure is necessary to change the body from the liquid to the solid state at a given temperature, than the pressure at which the body at the same temperature again spontaneously melts. Freezing almost always seems to take place at once; the corresponding fusion is apt to be prolonged, and in its gradual occurrence traces the contours of James Thomson's well-known, doubly inflected isothermals much more fully than does the allied case of vaporization.

The appearance of the cyclic parts of these isothermals is suggestive, and may be described in terms of their dimensions in the direction of volume and of pressure respectively. The former dimensions indicate the probable fate of the volume increment. They show that as pressure and temperature increase, the volume increment tends more and more fully to vanish and they thus imply a lower critical temperature at which the solid would change into the liquid continuously as far as volume is concerned. It does not

follow that other properties of the body would here also vary continuously. For naphthalene this point would lie in a region of several thousand atmospheres, and several hundred degrees Centigrade—therefore in a region too remote to admit of actual approach.

Again, the breadth of the cycles, measured along the pressure axis, decreases from the centre of the field both in the direction of increasing and decreasing pressures. The tenor of these results is an additional indication of the recurrence of a lower critical temperature at which cycles must necessarily vanish. The decrease of the breadth of the cycles in the direction of decreasing pressures suggest the possible occurrence of a point in the region of negative external pressure, so circumstanced that beyond it the substance would solidify at a lower pressure than that at which it fuses. This may be interpreted as follows: The normal type of fusion changes continuously into the ice-type of fusion through a transitional type characterized by the absence of volume lag.

An independent discussion more searching in character has quite recently been given by Tammann. Tammann points out that for the normal case of fusion and for increasing pressure, the two determinative factors of the Clapeyron equation—the volumes and latent heat of fusion—will not in general simultaneously become and remain zero. He argues that the volume constant will at the outset decrease with pressure passing through zero to negative values. Hence the curve representing the relation of melting point to pressure must initially rise to a maximum when the melting point pressure ratio is zero, and then decrease. Contemporaneously the latent heat of fusion, decreasing continually with pressure, eventually also reaches zero but at a much later stage than the volume constant. At this stage therefore, since melting point and the volume constant now have definite values(the latter negative), the melting point and pressure ratio is negatively infinite. Hence the curve expressing the relation of melting point to pressure decreases with increasing pressure from the maximum specified as far as the pressure at which latent heat is zero, and there drops vertically downward. Thus Tammann's melting point pressure curve, with its initial and final ordinate in the direction of temperature, maps out a field of pressure and temperature, within which the body is solid. Outside of this region the body is liquid and cannot by pressure alone be conceivably converted into the solid state. Any thermo-dynamic change involving a march through the boundary of this region is

accompanied by the discontinuity of fusion, of viscosity. etc. A march through the final ordinate (for which latent heat is zero) is probably not accompanied by such discontinuity. For a given temperature there may be two fusion pressures. At a temperature sufficiently below the melting point. the continued increase of pressure should therefore move the normally fusing body from the solid into the liquid state continuously. This is a somewhat anomalous result of close reasoning, but it must not be forgotten that in the depth of our ignorance of the actual occurrences above several thousand atmospheres, the term anomaly is a misnomer. Indeed, if we regard the melting pressure curve beyond the stated maximum as characterizing the ice type of fusion (which Tammann does not do) our difficulties would in a measure be reconciled.

Tammann finally points out that the term lower critical temperature is not justified by the character of the phenomenon. Data for melting point and pressure due to Damien seem directly to corroborate the occurrence of zero values in the ratio of melting point and pressure increments, but Damien's tests are restricted to a pressure interval much too small to be trustworthy. Of the two bodies which have been tested throughout long pressure intervals, naphthalene shows a linear melting point and pressure ratio for 2000 atmospheres, while the carbon tetrachloride of Amagat, though initially concave upward, soon also becomes linear. Clarence King has therefore in geological considerations so represented it. To conform with Tammann's inferences the interior of the earth would have to be a fluid.

One point of issue, however, in these cases is clear: At Andrews' critical temperature both the difference of specific volumes and the latent heat of fusion vanish simultaneously wherever observed. Under corresponding conditions of change from liquid to solid, the internal pressures are of tremendously greater value for both states, and the passage of the solid into the liquid molecule may involve an immense transfer of energy without any corresponding change of volume: for the density of the molecule itself eludes observation. The manner of this isothermal change from one state to the next is in all cases along the characteristic doubly inflected contour first pointed out by Thomson for vapors, and since elaborated by Van der Waals, Clausius and others. We may for brevity call this a *volume lag* and measure it in terms of the pressure or the volume interval subtended. The liquid can exist even

above the critical temperature, which would mean that even here pressure must be reduced below the critical pressure in order to rupture the liquid molecule.

Pronounced as these phenomena are for the change from gas to liquid, they become much more remarkable, indeed often formidable for the change from liquid to solid. In this case a volume lag subtending more than 100 atmospheres is the rule. In other words, it takes a much greater pressure to solidify a liquid at a given temperature than to liquefy the solid. Among all these cases there is a group of well-known bodies in which, while the solidification pressure is of marked intensity, the isothermal pressure of spontaneous fusion may even be below zero, or be in the region of negative pressure. Take the single example of thymol among many: This body between zero Centigrade and its melting point at 53°, can be kept in either the solid or the liquid state at pleasure. Given at about 50° in the liquid state it would require more than 2000 atmospheres to solidify it. Once solid it must obviously remain so even if pressure be wholly removed. But thymol may be similarly treated beginning with the undercooled liquid state at 28°, *i. e.*, 25° below its melting point. Even here at least one thousand atmospheres are needed to condense it (400 have been tried quite ineffectually). Once solid it would require about 1000 atmospheres of *negative* external pressure again to melt it. In other words, it could not be melted again on the same isothermal.

If we but knew more about the physical constants involved in these transformations, we could predict the results along the lines of J. W. Gibbs's splendid theory of the equilibrium of heterogeneous mixtures: but with the dearth of our concrete knowledge of long range physical phenomena relating to liquids and solids, we must be content with humbler methods.

I have always regarded the significant behavior instanced for the case of thymol, as capable of a broad interpretation. Profs. J. J. Thomson and Fitzgerald abroad and Elihu Thomson in this country have recently sought for atomic dissociation in the electrolyzed vacuum of a Crookes' tube. Speaking to the same point, I would venture to assert that we may reasonably look to the volume lag for a rational account of the genesis of atoms. We have already met with two orders of volume lag: the first at the mergence of gas into liquid being usually a few atmospheres in isothermal value; the second at the mergence of liquid into solid, a

hundred or even one thousand times as large in isothermal value, and characterized by the fact that, whereas freezing pressures may be enormous, the corresponding isothermal melting pressure may even be markedly negative.

If then we further inquire as to what will happen if we indefinitely compress the solid along a suitable isothermal, I think it is logically presumable that, with the succeeding and profoundly accentuated volume lag, we shall reach the next atom in a scale of increasing atomic weights.

However enormous the condensation pressure for this purpose may be, it is supposable, in the light of the examples already given that, along an accessible isothermal, the disintegrating external pressure of the new atom may be permanently negative. Hence the new atom will persist within the pressure and temperature range available in the laboratory.

But the last stage is virtually identical with the first, or the inherent nature of these changes is periodic. The inference is therefore that, under suitable thermal conditions and continually increasing pressure, the evolution of atoms, of molecules, of changes of physical state, again of atoms and so on indefinitely, are successive stages of periodically recurring volume lag.

PAPERS READ.

[ABSTRACTS.]

THE EFFECT OF HEAT ON THE ELASTIC LIMIT AND ULTIMATE STRENGTH OF COPPER WIRE. By Prof. FRANK P. WHITMAN, Adelbert College, Cleveland, O. and MARY CHILTON NOYES, Ph.D., Painesville, O.

IT was observed by Miss Noyes, while studying the effects of heat on Young's Modulus in copper wires, that the elastic limit appeared to be much smaller at high than at low temperatures.

The authors, taking up the matter more at length, experimented upon a number of wires under different conditions, finding that the elastic limit of copper wire changes from 20 to 40 per cent between the limits, approximately, of 15° and 115° C.

A similar decrease was found in the ultimate strength of copper wire, under similar circumstances. the breaking weight at 100° being from 5 to 20 per cent smaller than at 15°.

[This paper will be printed in the Physical Review.]

ARC SPECTRA. By Prof. ARTHUR L. FOLEY, University of Indiana, Bloomington, Ind.

BY means of a Rowland grating and Brashear mounting, and a concave mirror to throw an image of an arc upon the slit, a photographic study was made of the spectra of different portions of the electric arc. The outer yellow sheath of the arc gave a spectrum consisting chiefly of the lines of Ca and Fe, while the central violet region gave, in addition to more lines of these metals, a spectrum of which the most prominent feature was the three so-called cyanogen bands.

The lines of all the elements diminished in intensity as the slit was moved from the center of the arc to the outer edge, though the *rate* of fading out was not the same for different elements or for different lines of the same element. No lines were found having maxima in the outer sheaths.

The spectra of twelve elements were studied to determine the nature of the lines near the carbons and directly between them. The salts used were barium carbonate, sodium nitrate, the chlorides of zinc, calcium,

strontium, potassium and lithium, the sulphates of chromium, cadmium and aluminum and the oxides of rubidium and titanium. In every case the carbon lines clung to the positive pole and the metallic lines to the negative pole. Ca may be taken as an instance.

When the upper carbon only contained Ca and was made positive, all the Ca lines extended across the arc to the lower negative pole. When it was made negative, of the forty-seven Ca lines at the upper carbon only fourteen extended across to the lower positive carbon.

The following is a more conclusive illustration of the electrolytic nature of the arc:

Ca was placed in the *negative* carbon only of a horizontal arc and the carbons were placed 1cm. apart. The arc was started by bringing the two carbons into momentary contact by passing between them a third carbon. After one minute the current was shut off and a plain carbon substituted for the one containing the Ca. No Ca lines appeared (except those always present in the ordinary arc) when the arc was again formed and the spectrum photographed. Strong Ca lines appeared when the experiment was repeated under the same conditions except that the *positive* carbon was filled with the salt. The *Ca travelled freely with the current, but not against it.*

A Helios enclosed lamp was modified so that a gas could be introduced into the arc and globe through the lower hollow carbon. A study was made of the cyanogen bands at $\lambda = 3883$, $\lambda = 4216$ and $\lambda = 3590$, and of the carbon bands at $\lambda = 4737$ and $\lambda = 4382$.

After the arc had run a few minutes the wasting away of the poles was very slow and the strong cyanogen and carbon spectrum was almost free from metallic lines. The introduction of air, CO_2 or O increased the wasting of the poles and the brilliancy of the metallic spectrum.

With an arc between carbons which had been heated red hot in a vacuum, the cyanogen bands were almost, if not quite, as strong when the arc was in an atmosphere of CO_2 or sulphur vapor as when cyanogen was introduced into the arc and globe. That cyanogen may be decomposed in the arc was shown by the presence of the carbon bands when cyanogen was introduced into an arc between copper electrodes.

[This paper will be printed in the Physical Review.]

THE TRANSMISSION OF RADIANT HEAT BY GASES AT VARYING PRESSURES.

By CHARLES F. BRUSH, Cleveland, Ohio.

BEFORE describing my own investigations on the transmission of heat by gases, I shall refer briefly to the classical work of a somewhat similar nature by MM. Dulong and Petit early in the present century: "Researches on the Measure of Temperatures, and on the Laws of the Communication of Heat," Ann. Phil. 1819.

In their researches on the "Communication of Heat," Dulong and Petit used as the cooling body a very large thermometer bulb filled with mercury; and as the recipient of the heat, a large copper bulb or "balloon" about three decimeters in diameter, in the center of which the thermometer bulb was placed. The copper balloon was coated with lamp-black on the inside, and kept at any desired constant temperature by means of a water bath, or melting ice. The thermometer tube was of such length as to bring the zero of the scale outside the balloon; and the thermometer was adapted to be removed, heated, and quickly replaced, air tight. The balloon was connected with an air pump capable of rapidly exhausting it down to about two millimeters pressure; and also with a gas-holder from which it could be quickly filled with the gas whose cooling properties were to be determined. The rate or "velocity" of cooling of the thermometer bulb was deduced from observations of the falling temperature at equal intervals of time.

With this apparatus Dulong and Petit made many carefully conducted experiments at differences of temperature between the thermometer and balloon ranging as high as 300 degrees; and with several different gases besides air, ranging in pressure from atmospheric, to two millimeters. From the results of these experiments they deduced severals laws of cooling which they held to be general in their application. They sharply divided the cooling into two parts: that due to convection — the actual contact of the surrounding cooler gas renewed by its own currents; and that due purely to radiation — the same as would occur in an "absolute vacuum." They derived a constant value for the latter, and values for the former varying with different gases and different pressures. They generally used the thermometer bulb naked. with its natural vitreous surface, but sometimes they silvered it. While this radical change in the character of surface greatly changed the loss of heat due to radiation, it apparently had no effect on that due to convection.

MM. Dulong and Petit fell into the grave error of deducing the behavior of the last few millimeters of gas from that of the rest. In this way they arrived at the following "sixth law":—

"The cooling power of a fluid diminishes in a geometrical progression when its tension itself diminishes in a geometrical progression. If the ratio of this second progression is 2, the ratio of the first is 1.366 for air; 1.301 for hydrogen; 1.431 for carbonic acid; and 1.415 for olefiant gas."

My own observations show that this law can be approximately true only in the case of a large balloon; and at pressures from a few millimeters upward. There is no suggestion of it when a small balloon is used; and at small pressures it does not obtain with either large or small balloons.

It was through misplaced confidence in their sixth law, that Dulong and Petit were led to place a value on the rate or velocity of cooling in vacuo, something like a hundred per cent too high; and as they derived the cooling values of gases by deducting the cooling effect of a vacuum from the total cooling observed, all their values for gases are much too low.

Other experimentalists also have studied the transfer of heat by air and other gases at various pressures. Kundt and Warburg (Pogg. Ann., 1874-5) and Winkelmann (Pogg. Ann., 1875-6) observed that the rate of heat transmission remained substantially constant through a long range of diminishing pressure; and then decreased with further exhaustion. But as they made no measurements of pressure below one millimeter (1316 millionths of atmospheric pressure), their results have no quantitative value for low pressures.

Crookes, in his paper, "On Heat Conduction in Highly Rarefied Air" (Proc. Roy. Soc. 1880), described a similar experiment in which he carried the pressure measurements as low as 2M. (two millionths). From the fall in the rate of heat loss which occurred between the pressures of 760 millimeters and 1 millimeter, and 5M. and 2M. be concludes: "We may legitimately infer that each additional diminution of a millionth would produce a still greater retardation of cooling, so that in such high vacua as exist in planetary space the loss of heat — which in that case would only take place by radiation — would be exceedingly slow."

In this conclusion, Mr. Crookes was, I think, wrong. I find that the curve representing the rate of cooling does not break down materially, at pressures as low as a twentieth of a millionth.

My own investigations on "The Transmission of Radiant Heat by Gases at Varying Pressures," form part of a general study of the properties of high vacua, in which I have long been engaged.

In the course of my work it became necessary to know how much of the heat communicated by a good radiating body at ordinary temperatures, to a neighboring body at a slightly lower temperature, through an intervening gas, is transmitted by the so-called ether, and how much by the gas; and whether any of that transmitted by the gas is communicated otherwise than by the process of convection. Also why, and to what extent, do the gases differ from each other in their heat-transmitting capacities.

In the drawings herewith, Fig. 1 is a diagram of the apparatus used in my experiments. A is the thermometer whose cooling was observed. It has a very open scale divided into two-tenths degrees C. The zero point is placed a long distance (about 170 millimeters) above the bulb, for obvious reasons. The bulb is cylindrical, about 20 mm. long, and about 7 mm. in diameter, and is coated with lamp-black applied with a very thin alcoholic solution of shellac After several hours' baking at 100 degrees in a good vacuum, this bulb gave constant radiation results. The thermometer is suspended by a platinum wire, with its bulb in the center of the large pear-shaped glass bulb B, about 112 mm. in diameter The stem of the thermometer hangs freely in the long neck of the large bulb. I shall hereafter call the glass bulb, B, the "large radiation bulb," or simply the "large bulb," to distinguish it from a smaller one used later. The bulb B is surrounded by a copper tank C, lagged with woolen cloth, and filled with crushed ice and distilled water. A wire netting, C′, serves to keep some of the ice always below the lowest point of B. The tank C is movable on vertical guides, whereby it may quickly be raised to, or

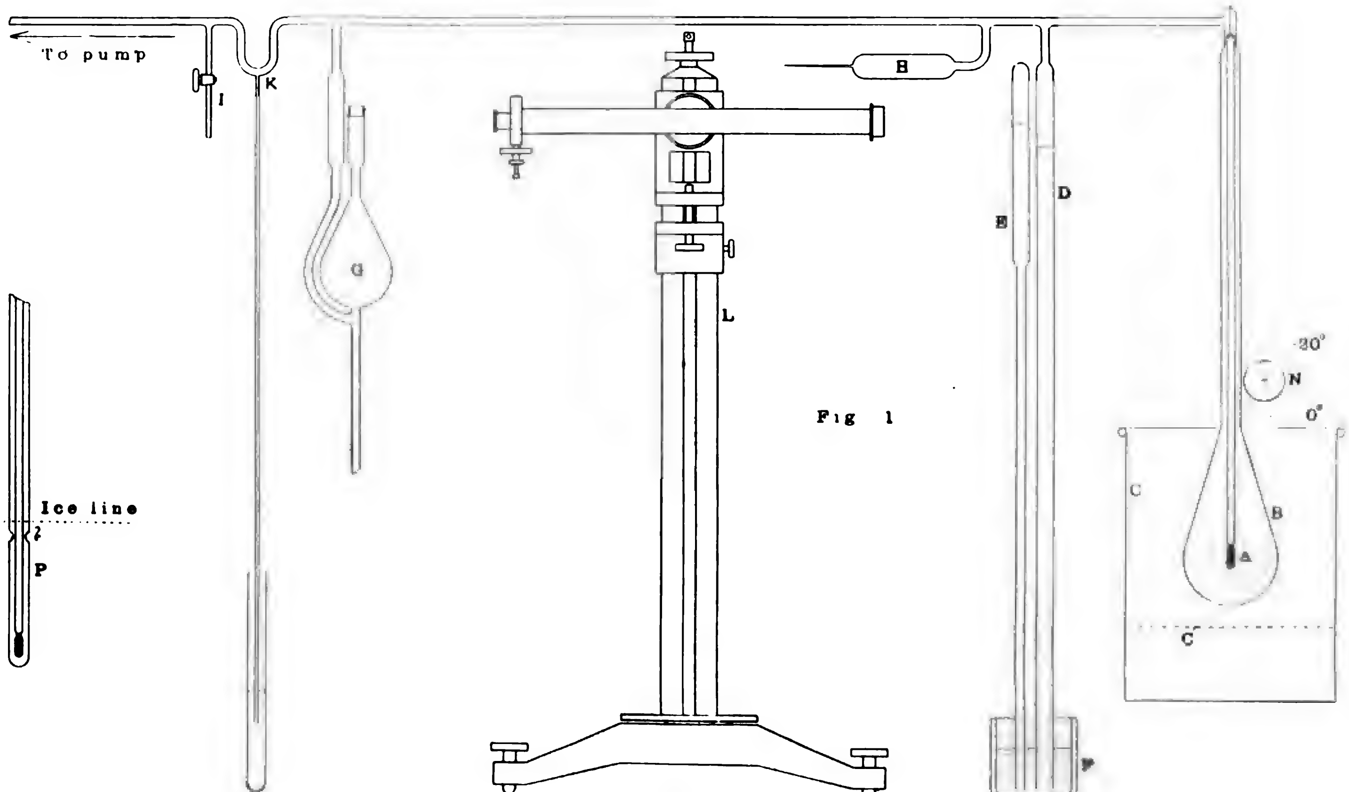
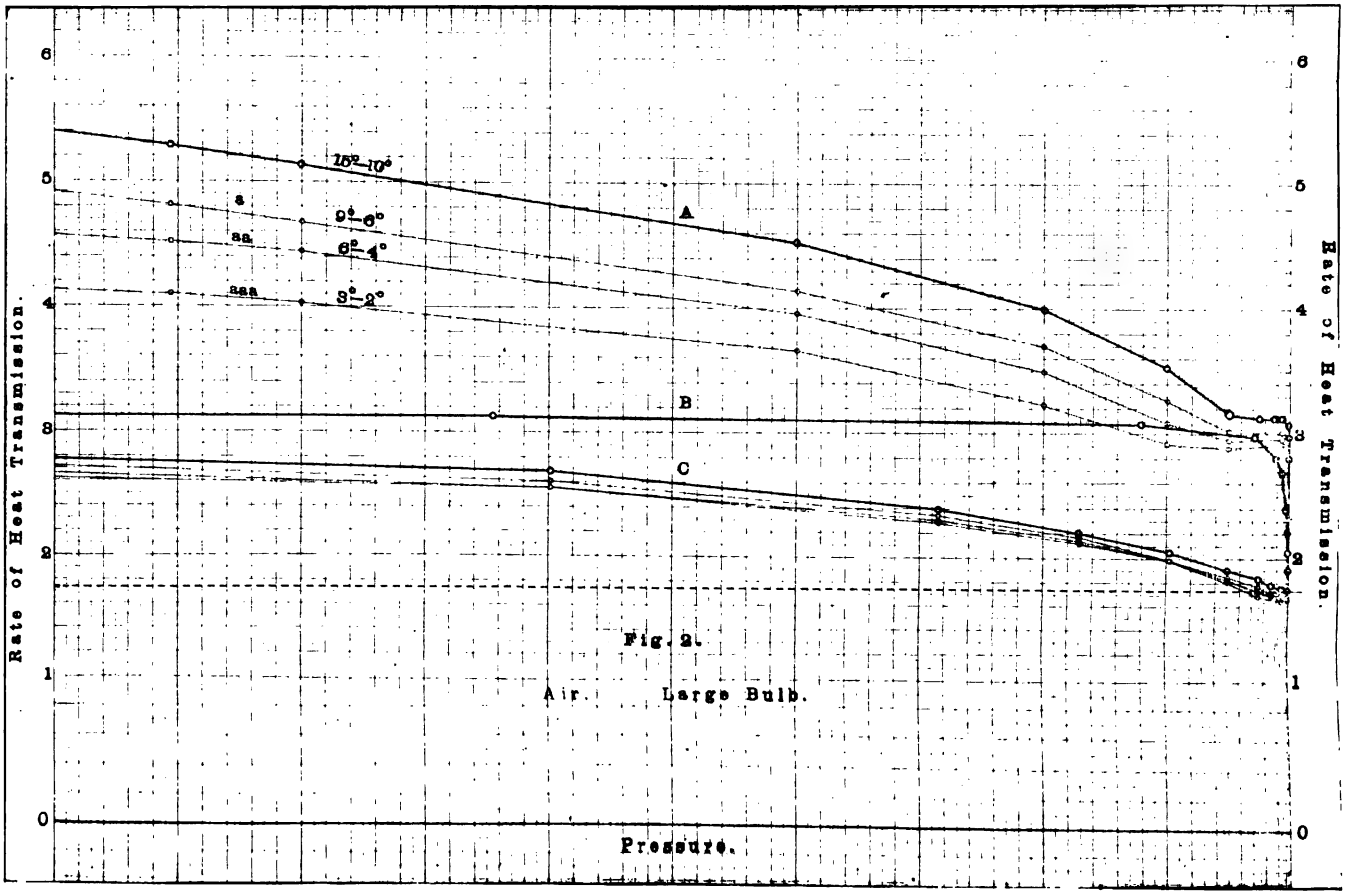
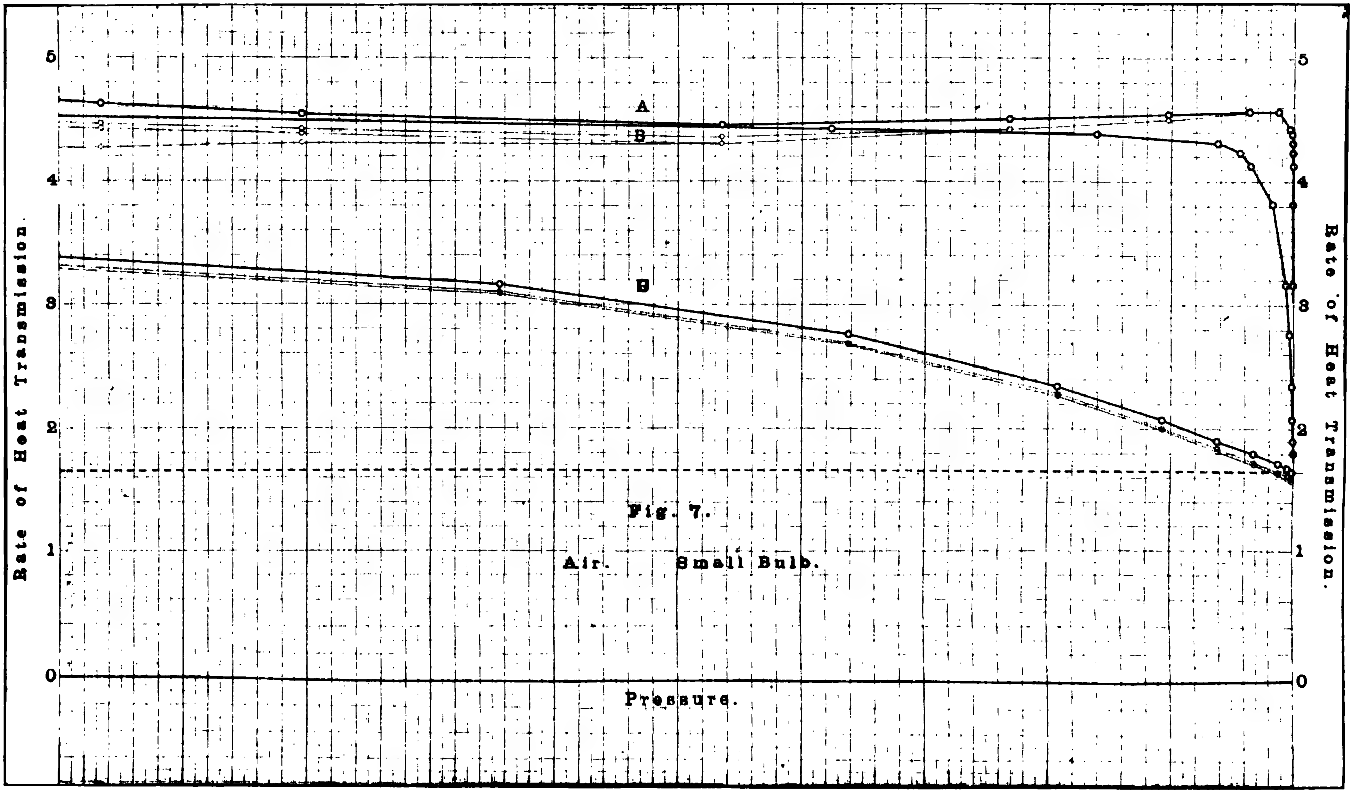


FIG. 1.



Rate of Heat Transmission.
Rate of Heat Transmission.
0
1
2
3
4
5
6
15°–10°
9°–6°
6°–4°
3°–2°
a
aa
aaa
A
B
C
Fig. 2.
Air. Large Bulb.
Pressure.

FIG. 2



Rate of Heat Transmission.
5
4
3
2
1
0
A
B
B
Fig. 7.
Air. Small Bulb.
Pressure.
Rate of Heat Transmission.

FIG. 3.

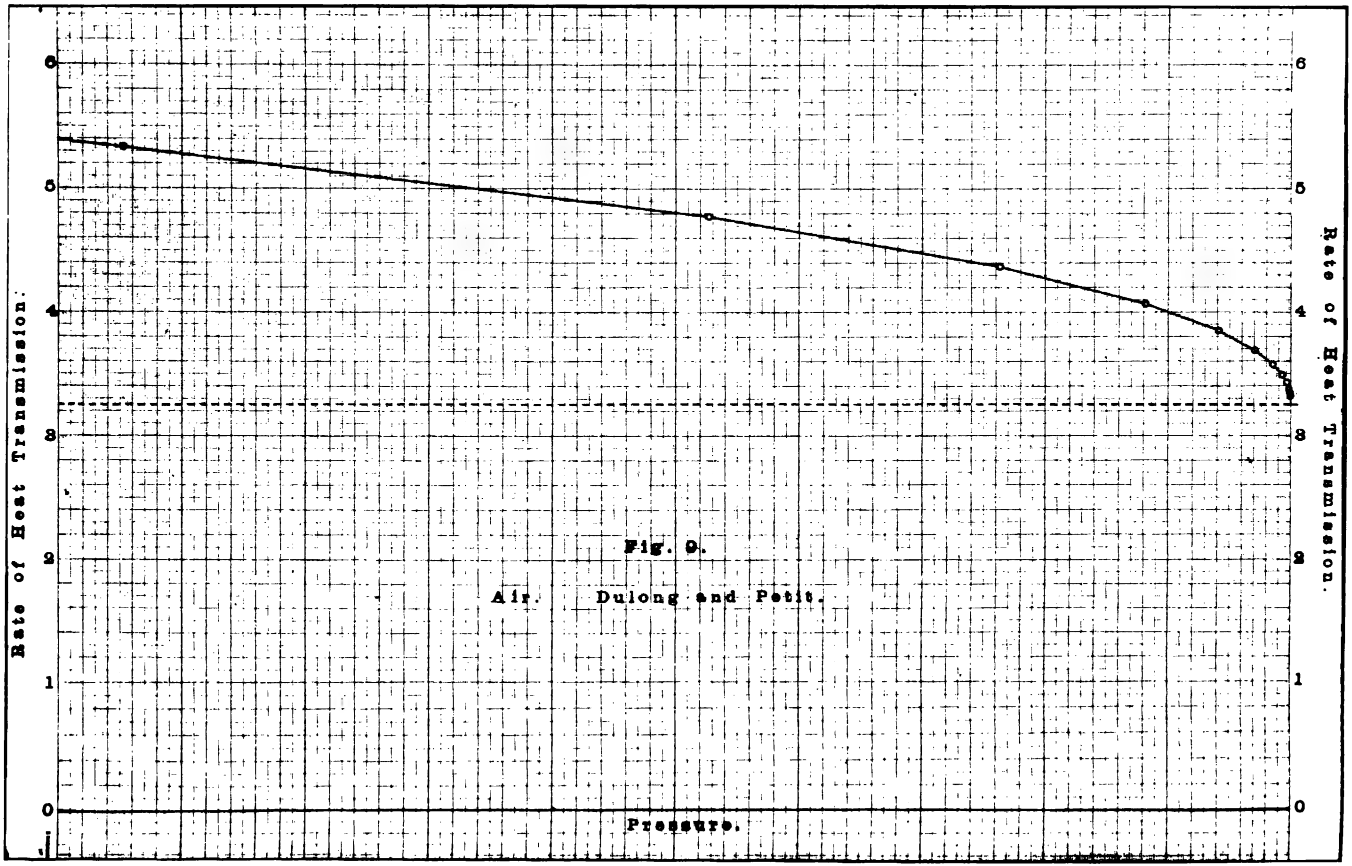

Rate of Heat Transmission.
Rate of Heat Transmission.
6
5
4
3
2
1
0
Fig. 9.
Air. Dulong and Petit.
Pressure.

FIG. 4.

lowered from, the position shown, thus exposing the bulb B alternately to the ice bath, and the atmosphere of the laboratory. The bulb B communicates freely with the large barometer tube D, which is used for measuring all but very small pressures. E is a standard boiled barometer, dipping into the mercury cistern F, common to both barometers. G is a McLeod gauge giving very accurate measurements of small pressures, and H is a drying bulb containing phosphorus pentoxide. The glass stopcock, I, serves to admit other gases than air. The mercury valve, K, prevents any leakage backward from the pump when the latter is stopped, during observations. Exhaustion is effected by an automatic Sprengel pump having five fall tubes. L is a fine cathetometer placed in front of the whole apparatus, and by rotation on its vertical axis is adapted to read the McLeod gauge, both barometers, and the thermometer. It has a vertically divided scale with vernier and microscope, for reading the barometers, and a micrometer for reading the gauge. A watch, N, is mounted close beside the thermometer on a sliding frame, so as to be easily kept in the field of view of the cathetometer telescope when the latter is used to observe the falling temperature.

Before using this apparatus, I always exhausted to a good vacuum, and heated the bulb B by means of a water bath, and all other vacuous parts by means of an air bath, to 100 degrees for several hours. This was found necessary in the first instance with air, in order to divest the inner glass surfaces of that portion of their coating of adherent gas most easily given off in a vacuum. This gas was pumped out; and, not being principally air, was not largely re-absorbed when air was admitted. Without this precaution I was unable to obtain constant results at very low pressures. When other gases were tried successively, the preliminary heating prevented gas from one operation attaching itself to the glass, and remaining to contaminate the succeeding gas at very low pressures.

I next introduced the proper gas up to atmospheric pressure, and made a preliminary cooling of the thermometer by raising the ice tank C. This preliminary cooling was found to have a slight effect on the readings next following, and was done to make the first set of readings on any day entirely comparable with the others. I then lowered the ice tank, and when the temperature had risen to 18 degrees, stirred the ice and water thoroughly, raised the tank again, and observed the thermometer through the telescope — noting by the watch N the instant when the falling mercury passed each degree of the scale. Then, with the ice tank still up, I noted the pressure by measuring with the cathetometer the difference in height of the barometer columns in D and E. The barometer D showed that the gas in the radiation bulb cooled nearly to zero with very great rapidity when the ice tank was raised. I always measured pressures with the radiation bulb cold. It was usual to repeat the whole operation to confirm results before reducing the pressure by the pump.

Observations were thus made at pressures varying from atmospheric down to the best vacuum obtainable. In some instances many series of observations were made at varying pressures all within the last millionth.

The gauge could be relied upon to measure these small pressures with very great accuracy: but it was difficult to maintain them long at any exactly constant value, on account of the continual, though slight, evolution of gas from the glass of the apparatus.

As I desired only comparative results, no correction was made for the probable slight inequalities in the calibration of the thermometer; nor for heat conducted to or from the bulb by the stem; nor for the change of zero point due to changing external pressure. The mercury fell exactly to zero at atmospheric pressure, and about one-fiftieth of a degree lower at no pressure. The pressure error due to differences of capillary depression in the two barometers was ascertained at high exhaustions, and found nearly constant. It was always corrected. The different gases used were carefully prepared and dried, and were introduced quite free from any admixture with air.

My observations have extended over a long period, and are far too voluminous to be recorded here in detail. But I have embodied their most salient features in a series of curves which render them readily apparent to the eye. In these curves the abscissæ represent the pressure, and the ordinates represent the rate of heat transmission through the gas, from the thermometer bulb to the ice-cold envelope. The rate of transmission at any particular pressure is expressed by the reciprocal of the number of seconds required for the temperature to fall through a given number of degrees. For convenience of scale, all the reciprocals are multiplied by 500.

Fig. 2 shows the curve for air. The heavy line represents the rate of cooling from 15 degrees to 10 degrees. It is in three sections, A, B and C. Section A embraces the whole range of pressure from nothing to atmospheric; section B embraces the range of pressure from nothing to .01 of atmospheric; and section C embraces the range of pressure from nothing to .0001 of atmospheric, *i. e.*, 100M. (one hundred millionths). Atmospheric pressure is taken at 760 mm. Thus it will be seen that section B is the last hundredth of A, magnified a hundred times; and section C is the last hundredth of B, magnified a hundred times. This magnification of the abscissæ without change of the ordinates, enables us to study every part of the curve with ease. The small circles represent the points in the curve established by observation. These points are shown exactly as found, without any attempt to smooth out rough places in the curve. The same is true of the curves of other gases. The heavy dotted line, parallel with the base, indicates that portion of the total heat transmission due to the ether; while all above it represents that due to the air.

Starting at the left-hand end of section A, representing the rate of heat transmission at atmospheric pressure, we observe that the curve drops regularly at a rate faster than the diminution of pressure, during ninety-five per cent of the whole range of pressure from atmospheric to zero. Beyond this point the rate of heat transmission remains substantially constant, as shown by section B and the latter part of A, down to a pressure of about .000 3—a range of nearly ninety-nine and a half per cent of that

remaining. Here the curve suddenly begins to drop again, and falls steadily, as shown by section C and the latter part of B, until it meets the ether line at the zero of pressure.

Under the curve A, I have drawn curves with finer lines, representing the rate of heat transmission at smaller differences of temperature between the thermometer and ice bath. As before stated, A represents the cooling from 15 degrees to 10 degrees. On the same scale, a represents the cooling from 9 degrees to 6 degrees; aa, from 6 degrees to 4 degrees, and aaa, from 3 degrees to 2 degrees. Now, Newton's law of cooling requires that the rate shall vary directly with the difference of temperature between the cooling body and the surrounding medium. While this law is known to be incorrect for large differences of temperature, it is generally accepted for very small differences. If it were correct under the conditions of the present experiment, then the ratios of the times required for the temperature to fall through the several ranges above indicated, would all equal unity, and the curves A, a, aa, aaa would coalesce. But they are very far from doing this. It will be observed that all of these curves preserve their relative values very closely indeed, until they approach the point of pressure where the curve A reverses itself; then they begin to bunch themselves very much closer together, especially the lower ones, and shortly reach a greatly reduced, as well as varied ratio of values which they retain substantially unchanged to the end, as shown in connection with section C. To avoid confusion of lines, I have omitted the secondary curves corresponding with section B.

Carbon monoxide was chosen for comparison with air, because its absorptive power for radiant heat is many times greater, while its specific heat is almost exactly the same. The principal curve, representing the rate of heat transmission from 15 degrees to 10 degrees, differs very little from that of air. It shows a slightly better rate than air at very small pressures; not quite so good a rate as air at intermediate pressures; and the same rate, at atmospheric pressure. But the curves a, aa, aaa, representing equivalent amounts of cooling at smaller temperature differences, are materially unlike those of air. At high pressures they have about the same ratio values as with air; but the ratio diminishes much less at intermediate and low pressures; that is to say, the curves remain further apart. It is equally noticeable that the curves aa, aaa, retain their full relative ratio values at low pressures, while with air, they nearly coalesce.

It was thought that ethylene might transmit heat more rapidly than air, because of its much higher specific heat. But it does not do so. Its curve has the same form as those of air and carbon monoxide. It transmits heat nearly as well as air at atmospheric pressure, but not nearly so well at intermediate pressures. At a very few millionths, however, it conducts a trifle better than air. The curves a, aa, and aaa, have the same characteristics, and about the same ratios, as those of carbon monoxide.

Hydrogen was next tried, on account of its very low coëfficient of viscosity, as well as its very high specific heat. While in general form the hydrogen curve resembles the air curve, all the ordinates are immensely

increased. It is noticeable that the intermediate section B of the curve lies much nearer A than C, quite different from its relative position in the curves of the other gases. This section of the curve shows that hydrogen retains about two-thirds of its initial heat transmitting power at a pressure nearly two hundred times smaller than does air. The curves A, a, aa, and aaa, have something like the same ratios as they have in the cases of carbon monoxide and ethylene. In general, it may be said of hydrogen in the large radiation bulb, that it transmits heat nearly four times as fast as air at atmospheric pressure; more than twice as fast at a very few millionths, and more than seven times as fast through a long range of intermediate pressures.

As evidence of the accuracy of the observations on which the curves thus far described are based, it is gratifying to note that the vacuum, or ether line, locates itself exactly the same in all.

In making the above described observations, I looked for some change in the phenomena when the exhaustion reached the point at which the mean free path of the gas molecules equaled the distance between the thermometer bulb and the cold walls of the enclosing globe. This should have been at a pressure of about two millionths. No such change was observable, however, in any case. Partly in pursuance of the same idea, I resolved to repeat some of my experiments, using a very much smaller radiation bulb. This I expected would also reduce that portion of the total cooling effect due to convection currents. I accordingly employed the bulb or tube P, Fig. 1, in my further experiments. This is made from a thin glass tube slightly less than 20 millimeters internal diameter, and in it hangs the same thermometer A, which was used before. In transferring the thermometer, great care was taken to avoid any disturbance of the coating of lamp-black on its bulb. At b, is a contraction of the tube P, to prevent the thermometer bulb swinging against the inside of the tube. The contraction b is, however, much larger than the thermometer stem, so that normally the latter does not touch it. The thermometer bulb hangs exactly in the center of P, near its bottom, and is separated from it by a space of a trifle more than six millimeters—almost exactly a quarter of an inch; instead of two inches, as in the case of the "large bulb." The tube or bulb P, I shall hereafter designate the "small radiation bulb," or simply "small bulb," to distinguish it from the large one.

The curve for hydrogen, with the small bulb, differs radically in size and form from that obtained with the large bulb. Section A, instead of drooping rapidly with decreasing pressure, maintains almost its full value throughout. Section B starts with nearly double its old value, but breaks down much earlier. Section C starts with a little higher value, but is much straighter, and consequently has a lower value throughout most of its length. The curves a, aa, aaa, are very peculiar. They start at atmospheric pressure with much smaller total, and very different relative ratios than before, and are successively absorbed into A. They reappear later, however, but with small ratios.

Fig. 3 gives the curve for air, with the small bulb. It differs from that

with the large bulb quite as much as did the hydrogen curve. Section A droops slightly, and then regains almost its full atmospheric value at one per cent pressure. Section B has the same form as with the large bulb (Fig. 2), but more than double its value; and section C also has a much higher value throughout. The curves a, aa, aaa, have small ratio values at the beginning, and are absorbed into section A the same as with hydrogen. But aa, and aaa, coalesce when they reappear, and coincide to the end; while the ratio between a, and aa, remains constant at a very small value.

The curve for carbon dioxide, with the small bulb, closely resembles the air curve in form, but has a very much smaller value throughout. While the curves aa, and aaa, are soon united, and remain so to the end, a, and aa, never disappear as they did in the cases of hydrogen and air.

With the small bulb, as with the large, no change in the character of the phenomena was observable when the exhaustion had reached the point at which the mean free path of the molecules equaled the space through which the heat was conducted. This point was reached in the small bulb at a pressure of about fourteen millionths.

It seems reasonable to assume that the radical difference between sections A of the curves obtained with the large and small bulbs respectively, was due to an almost complete suppression of convection currents in the latter case. In the absence of convection currents, that part of the heat transmitted by the gas, was probably carried by a process analogous to conduction in solids. The shortness of conductor, in the case of the small bulb, may account for the greatly increased rate of conduction. But why the conductivity of a gas remains nearly constant through a very wide range of pressures, is not clear. Mr. Crooke's explanation of this phenomenon seems to me very unsatisfactory.

It will be noticed that the "ether line" is about four per cent lower with the small bulb than with the large one. This may be due to the greatly decreased amount of surface presented by the small bulb for absorption of the radiant heat.

The enormous heat conducting capacity of gases, at very small pressures, is strikingly shown in all the curves. But hydrogen is preëminent in this respect. Thus, in the large bulb, hydrogen at a pressure of only twenty-six millionths of an atmosphere, transmits heat as rapidly as the ether! At seventy-six millionths, it equals air at atmospheric pressure; that is to say, it does the work of nearly two hundred thousand times its weight of air!

It is remarkable that at pressures up to a few millionths, all of the curves are nearly straight lines. This is especially noticeable in the small bulb curves; showing that at these small pressures the heat transmitting power of a gas varies directly with its amount. Hence it seems reasonably certain that, if the very small fraction of a millionth of the gas examined, which remained at the end of each experiment, could have been entirely removed, the heat transmitting power of the vacuum would not have been materially diminished. It was customary at the end of the

experiments with each gas, to close the gauge permanently when the pressure had fallen to a tenth of a millionth or so; and, with the capacity of the whole apparatus thus reduced, run the pump continuously from one to two hours. Several sets of observations were always made during this extreme exhaustion: and while the change in the rate of cooling of the thermometer was generally appreciable, it was always very small indeed. In my earlier experiments I took the greatest care to insure the absence of mercury vapor in the final vacuum. But the presence or absence of mercury vapor made no difference distinguishable from the errors of observation.

Of course the best vacuum producible by a Sprengel pump, still contains many thousands of millions of gas molecules per cubic centimeter. This may be regarded as a prodigiously large, or exceedingly small quantity of gas, according to our point of view. While it has no apparent effect on the general heat transmitting capacity of the vacuum, it does seem to interfere with or modify some function of the ether. This is the only explanation of certain phenomena that I can offer. I refer to the different behavior of the vacua with different residual gases, and in different sized bulbs, in the matter of adherence to or departure from, Newton's simple law of cooling. The curves a, aa, aaa, illustrate these differences in the several cases at the extreme end of section C of the principal curves. These differences are too large to be attributed to errors of observation. This is one of several reasons which lead me to suspect that, at higher pressures, all the gases examined interfere materially with, and retard the transmission of heat by the ether. In other words, I suspect that the dotted ether line of my curve sheets should not be drawn parallel with the base, and have a constant value at all gaseous pressures as shown, but should have a decreasing value as the gas pressure rises from zero. On this interesting phase of my subject I hope to have more to say at a future date.

Fig. 4 is an air curve plotted from figures given in Dulong and Petit's paper. It is drawn to such a scale that the rate of heat conduction at atmospheric pressure is the same as in my own experiment with air in the large bulb, and illustrated in Fig. 2. The first five stations in the curve are the ones from which they deduced their "sixth law" of cooling. The rest of the curve is drawn in accordance with that law, and the vacuum line represents exactly the value they assigned to the cooling power of an absolute vacuum. Comparison with Fig 2 shows how much they erred in their deduction.

A study of the curve embodying results obtained with a mixture of three volumes of hydrogen, and five volumes of carbon dioxide in the small bulb, shows that the carbon dioxide interfered very greatly with the performance of the hydrogen. Before any exhaustion was made, the hydrogen *alone*, would have done more than three times the work of both gases. It was not until the pressure had fallen to about 100 millionths that both gases combined did as well as the hydrogen would have done alone. Below this pressure, both gases contributed to the result.

This interference of mixed gases is a very interesting phenomenon; and seems to warrant the careful investigation which it is my intention to give it.

[This paper will be printed in the Philosophical Magazine.]

NOTE ON THE ELECTRIC CONDUCTIVITY OF CERTAIN SPECIMENS OF SHEET GLASS WITH REFERENCE TO THEIR FITNESS FOR USE IN STATIC GENERATORS. By Prof. DAYTON C. MILLER, Cleveland, Ohio.

IN connection with the construction of static electric machines, it was desired to choose from several specimens of sheet glass the one having the least conductivity. Sheets of the glass twelve inches square were silvered on the center of each side over an area of six inches square. By joining the ends of a 500 volt circuit, containing a large Wiedemann galvanometer with Braunn's guard ring, to these silvered areas, a very considerable galvanometer deflection was secured. It was thus shown that "cathedral glass," containing no lead, had about seven times the conductivity of common window glass which contains lead. Further experiments, in connection with chemical analyses and surface conductions, are to be undertaken.

ON THE COËFFICIENT OF ABSOLUTE RIGIDITY OF QUARTZ FIBERS AND ITS TIME AND TEMPERATURE VARIATIONS. By SAMUEL J. BARNETT, Cornell University, Ithaca, N. Y.

[*Communicated by Prof. E. L. Nichols.*]

THE immediate object of these measurements was to find whether or not a quartz fiber would afford a suitable suspension for the magnet of the great tangent galvanometer in the Physical Laboratory of Cornell University, when that instrument was mounted as a villari torsion galvanometer. The coëfficient of absolute rigidity was determined by the method of torsional vibrations, cognizance being taken of the conical form of the fibers. Six fibers were tested and the mean value of the measurements of the six showed a coëfficient equal to $1.46 \times 10.^{11}$ The individual values were in agreement with one another, showing no such wide variations as had been observed by Threlfall in his investigations with finer fibers. The mean value here given is very much smaller than the values obtained by Threlfall and Boys, apparently indicating that this coëfficient, like the longitudinal strength (Boys and Threlfall), increases relatively as the diameter diminishes.

To test the effect of time upon the period of vibration, four fibers were mounted on Dec. 22, 1896, and nine complete sets of observations were made extending until May 10, 1897 No time effect on the periods was observable. Care was taken to reduce these observations to a constant temperature of 21° C.

The influence of temperature upon the rigidity of quartz fibers had been previously investigated by Threlfall and Adair. This determination involves a knowledge of the coëfficient of linear expansion, for which constant the above mentioned authors obtained widely discordant results, ranging from +.000037 to —.000022. Measurements of the coëfficient of expansion in the present case gave concordant results, the mean value of which was +.0000003 for the linear coëfficient between 24°C. and 98°C. This coëfficient is so extremely small that it may be neglected in the reduction formula. As the result of observations upon the torsional period at different temperatures it was found that the rigidity of fused quartz increases with the temperature. The mean value of the coëfficient obtained from concordant observations upon seven fibers was found to be +.000115; while the elasticity of all other substances hitherto investigated, including rubber, has been found to *diminish* as the temperature increases.

[This paper will be printed in The Physical Review.]

THE DISCHARGE OF ELECTRIFIED BODIES BY X-RAYS. By DR. CLEMENT D. CHILD, Cornell University, Ithaca, N. Y.

THE writer's experiments deal chiefly with the dependence of the discharging effect produced by x-rays upon the pressure of the air surrounding the charged body. The rays were allowed to pass between a grounded zinc plate and a charged zinc box which almost completely surrounded it. Both plate and box were enclosed in the receiver of an air pump. The rate of discharge was determined by the fall of potential produced by the action of the rays in a given time. Numerous precautions were taken to guard against disturbances due to the direct electro-static action of the tube, and to the action of the rays upon the atmosphere of the room. The rays entered the receiver through a window of paraffined wood, which, on account of its high transparency, permitted more intense rays to reach the interior than have heretofore been used in such experiments.

With low potentials (6-15 volts) it was found that the rate of discharge at first increased with the pressure of the air, but finally attained a maximum and diminished again as the pressure was made still greater. The pressure corresponding to the maximum rate of discharge appeared to be very nearly proportional to the potential of the charged body. If this potential was as high as 35 volts the maximum was no longer found within the range of pressures used, and the rate of discharge was nearly proportional to the square root of the density. The fact that other experimenters have not found the maximum above mentioned is probably due to the fact that they did not use sufficiently intense rays. The writer has repeated his experiments by a modified method with the same results.

[This paper will be printed in The Physical Review.]

On the luminosity of pigmented surfaces under different sources of illumination. By Prof. Frank P. Whitman, Adelbert College, Cleveland, O.

This paper embodies the results of measurements, with the Flicker Photometer, upon the luminosities of a given set of pigmented surfaces, when lighted successively from various sources.

The luminosities were expressed as percentages in terms of a white surface exposed to the same illuminant.

The results were exhibited in curves, which made it easy to compare the character of the different lights used.

Sets of readings were also made, upon the same set of pigments, by two color-blind persons, one red-blind, the other green-blind.

The readings were as definite as those of the normal eye, but the curves differed notably; one showing abnormal brightness in the red and deficient illumination in the green, the other just the reverse, and to nearly the same amount.

[This paper will be printed in The Physical Review.]

Design, construction, and test of a 1250 watt transformer. By Prof. Henry S. Carhart, University of Mich., Ann Arbor, Mich.

Two of my students undertook, under my supervision, the design and construction of a transformer to determine how nearly theory dictates correct practice. They chose the core type instead of the shell chiefly for construction reasons, as the iron for the core type can be cut at the tinsmith's without a die. At the same time this type departs so widely from the common design that they were thrown more on their own resources.

In the core type the number of turns of wire is greater than in the shell type if the copper and iron losses are to be nearly equal to each other, This is the condition for highest efficiency. Since the primary and secondary losses are nearly equal, represent the entire copper loss by aI^2R and the iron loss by b. Then for highest efficiency $\frac{aI^2R + b}{EI} = c$ must be a minimum. E, I, and R are $E.\ M.\ F.$, current, and resistance of the secondary. Hence $\frac{dc}{dI} = \frac{aR}{E} - \frac{b}{EI^2} = 0$ for a minimum. Therefore $aI^2R = b$, or the copper and iron losses are equal.

For high efficiency under a light load, the number of turns of copper must be increased. But this causes a larger drop in potential and so impairs the regulation.

A. DESIGN.

Preliminary trials on paper showed that the number of primary turns should not be less than 1200 and the secondary 120 (or 60 in parallel). To determine the cross section of the iron, it is easily shown that

$$A = \frac{E}{4.44\ nNB},\ \text{or, in this case,}$$

$$A = \frac{1010 \times 10^8}{4.44 \times 135 \times 1200 \times 3000} = 46.8 \text{ sq. cm} = 7.25 \text{ sq. in.}$$

Allowing 10 per cent for oxide, the width of the core plate is 2.95 in.

On the basis of 1000 amperes per sq. in. the primary was made of No. 17 *d. c. c.* wire and the secondary of No. 8. Winding the secondary in two layers of 30 turns each on each spool, the length of the core plates becomes 7.848 in.

The estimated length of the secondary was 124.8 ft. and its resistance at 40° C., 0.0847 ohm. For the primary the length was 1602.6 ft, and its resistance 8.8 ohms.

The copper losses at full load are then 14.87 watts for the primary and 13.23 for the secondary. The hysteresis and eddy current losses were both combined under Steinmetz's formula with a hysteretic constant 0.0025. The indicated iron loss was 39.11 watts in a volume of 193.27 cu. in. and a flux density of 3000 lines per sq. cm. Hence the total estimated losses were 67.21 watts, and the efficiency 94.9 per cent.

The magnetizing current was computed by Thompson's formula as 0.048 ampere. The power component to supply the loss by hysteresis and eddy currents was 0.039. The total no load current is the resultant of these two in quadrature, or 0.061 ampere.

B. CONSTRUCTION.

Wheeling iron 15 mils. thick was used for the core. It was carefully annealed out of contact with the air by heating in a furnace to 923°C. and then slowly cooling. The above temperature is higher than the critical one at which iron ceases to be magnetic.

After winding and drying, the transformer was assembled by interlarding the end plates between those of the cores and compressing between ribbed clamps with the bolts through projecting ears.

C. TEST.

The resistance of the primary at 40°C. was found to be 8.46 ohms. That of the secondary, 0.883. The weight of the primary was about 9.25 lbs. and of the secondary, 6 lbs.

The iron loss, measured in the usual way by a Weston wattmeter was found to be 34.5 watts.

The regulation was determined by a method of comparison by means of two voltmeters, and the voltage from no load to full load varied only 2.3 per cent.

The cross-flux or magnetic leakage on full load was 2.65 per cent. The details of this measurement were given.

The following efficiencies were found by test: Load of 371 watts, 90.93 per cent; 742 watts, 94.42; 1020 watts, 95.12; 1286 watts, 95.37; 1475 watts, 95.43.

The comparison between observed and computed values is interesting:

	Observed.	Computed.
Resistance of primary at 40°C. . .	8.46	8.80
" " secondary " . .	0.083	0.0847
Copper loss, primary, full load . .	14.07	14.87
" " secondary " . .	13.07	13.75
Iron losses	34.5	39.11
No load current.	0.05	0.061
Efficiency at full load	95.4%	94.9%

ELECTROLYTIC ACTION AND INSULATION-RESISTANCE OF A COMMERCIAL CONDENSER. By Dr. K. E. GUTHE, University of Michigan, Ann Arbor, Michigan.

THE insulation-resistance of cables or absorbing condensers is a function of the time. It increases with the time very rapidly and finally approaches asymptotically a maximum. This is principally due to the soaking in of the charge which in the usual methods employed is taken into account as a quantity going through the dielectric. If there is electrolytic action in a condenser we may consider it as a high resistance cell and determine its resistance without any absorption from the outside taking place during the test. Experiments made on such a condenser, finding its resistance from the formula

$$r = \frac{E - E'}{E'} R$$

gave the following rules:

1. In condensers, in which there is some electrolytic action — and this was found to be the case in several absorbing commercial condensers — the resistance increases with the time, during which the circuit is closed.

2. The resistance depends also on the strength of the current passing through the condenser, being the smaller, the larger the current.

The great similarity between these results and those obtained with common batteries, especially dry cells, was pointed out and the conclusion was drawn, that in ordinary batteries the variation of the resistance depends largely on the solvent, in which the electrolyte is contained.

Corresponding experiments with electrolytic resistances showed that the action of the dielectric influences the result, the resistance being the larger the slower the alternations of the varying current and still larger in Strond and Henderson's constant current method.

[This paper is printed in the Electrical Engineer, Sept. 16, 1897.

THE GRAPHICAL TREATMENT OF ALTERNATING CURRENTS IN BRANCHING CIRCUITS WITH SPECIAL REFERENCE TO THE CASE OF VARIABLE FREQUENCY. By DR. HENRY T. EDDY, University of Minnesota, Minneapolis, Minn.

SUPPOSE at first, for the sake of simplicity, that one branch contains a condenser and the other a self inductive resistance. Then

$$\tan \theta_1 = \frac{1}{CR_1\omega}, \text{ and } \tan \theta_2 = \frac{L\omega}{R_2},$$

as is well known, express the lead and lag of the branches. These tangents of the lead and lag may be regarded as a special kind of curvilinear coördinates by which the total current in the two branches is determined; and the diagram in case a series of different constant values of these coördinates is assumed, consists, as appears from the well known graphics of the subject, of two series of intersecting semicircles. But the assumption of constant coördinates covers the case of constant frequency and is not usually of convenient application in case the frequency varies.

The author has assumed the product of the tangents, which is independent of the frequency, as a function of the coördinates which, plotted for a series of different constant values, would yield a diagram such as to permit the easy study of the total current in many cases of variable frequency. The remarkable properties of the series of curves thus obtained are treated at length in this paper, and special propositions are developed which facilitate their construction graphically.

Next, for simplicity, assume that both branches contain a condenser, or both an inductive resistance. Then

$$\tan \theta_1 = \frac{1}{C_1R_1\omega}, \text{ and } \tan \theta_2 = \frac{1}{C_2R_2\omega}$$

$$\text{or } \tan \theta_1 = \frac{L_1\omega}{R_1}, \text{ and } \tan \theta_2 = \frac{L_2\omega}{R_2}$$

Here the quotient of these coördinates is a function independent of the frequency, and the loci for a series of different constant values afford a diagram of special use in the cases under consideration. These curves, *i. e.* those for different constant ratios of the coördinates, are fully studied in the paper.

Furthermore, the loci arising in cases in which the product of the coördinates and their quotient assumed constant is not independent of the frequency are taken up for study and fully treated, whereby it becomes possible to consider the general case of two branches containing condensers and inductive resistances together, and to analyze the effect of variable frequency upon the total current, although the case is necessarily much more complicated than are the special cases previously treated.

ON SIMPLE NON-ALTERNATING CURRENTS. By Prof. A. MACFARLANE, Lehigh University, S. Bethlehem, Pa.

THE simple alternating current is expressed by the sine function $A \sin nt$, and in the theory of the subject many of the results involve the assumption that the alternating current is of that simple character. The question arises, What is the corresponding simple non-alternating current? At first sight it looks like $A e^{-nt}$, but it is more accurately the hyperbolic sine function $A \sinh nt$. The sine function may be considered as the vertical projection of a uniform circular motion; in a similar manner the hyperbolic sine function is the vertical projection of a motion along the equilateral hyperbola when equal areas are swept out by the radius-vector in equal times. When t becomes greater, this function becomes infinite, consequently the important case is where the function is affected by a decreasing exponential function, such as $A e^{-rt} \sinh nt$ where r is greater than n. This is the function which expresses the non-oscillating discharge of a condenser. The more general form is $A e^{-rt} \sinh (nt + \varphi)$ which corresponds to the oscillating discharge

$$A e^{-rt} \sin (nt + \varphi).$$

Two oscillating discharges, if they have the same exponential factor, are combined as two simple alternating currents having the same period. Consider the addition of two non-oscillating discharges having the same exponential factor and the same hyperbolic period. The problem is to

reduce $A e^{-rt} \sinh nt + B e^{-rt} \sinh (nt + \varphi)$.

$$\text{Sum} = e^{-rt} \left\{ (A + B \cosh \varphi) \sinh nt + B \sinh \varphi \cosh nt \right\}$$

$$= e^{-rt} \sqrt{(A + B \cosh \varphi)^2 - (B \sinh \varphi)^2} \sinh \left\{ nt + \tanh^{-1} \frac{B \sinh \phi}{A + B \cosh \phi} \right\}$$

$$= e^{-rt} \sqrt{A^2 + B^2 + 2 AB \cosh \varphi} \sinh \left\{ nt + \tanh^{-1} \frac{B \sinh \phi}{A + B \cosh \phi} \right\}$$

Hence the amplitude is $\sqrt{A^2 + B^2 + 2\ A\ B\ cosh\ \varphi}$ and the hyperbolic angle is $nt + tanh^{-1} \frac{B\ sinh\ \phi}{A + B\ cosh\ \phi}$

Hence corresponding to

$$A \underline{/0} + B \underline{/\varphi} = \sqrt{A^2 + B^2 + 2\ A\ B\ cos\ \varphi}\ \underline{/\ tan^{-1} \frac{B\ sinh\ \phi}{A + L\ cos\ \phi}}$$

we have

$$A \underline{/0} + B \underline{/\varphi} = \sqrt{A^2 + B^2 + 2\ A\ B\ cosh\ \varphi}\ \underline{/\ tanh^{-1} \frac{B\ sin\ \phi}{A + B\ cosh\ \phi}}$$

The rule for the graphical construction is also different from that for the alternating case and is as follows: — On $A + B\ cosh\ \varphi$ as diameter describe a semicircle; from the end of the diameter place $B\ sinh\ \varphi$ so that its other extremity meets the circle; join that point with the other extremity of the diameter; the line so found represents in magnitude and hyperbolic phase the sum of the two non-alternating currents.

STEREOSCOPIC VIEWS OF SPHERICAL CATENARIES AND GYROSCOPIC CURVES. By Prof. A. G. GREENHILL, Royal Military Academy, Woolwich, England.

I HAVE the honor to bring before this section some stereoscopic views which I beg to hand round. Those who are interested in the analysis will find the mathematical formula printed on the back. It will be sufficient to say that Abel's Theory of the Pseudo-Elliptic Integral has been utilized to construct some oases, as we may call them, in the desert of elliptic functions required for the general solution.

I would direct attention to the tessellated pavement which appears at a definite position; this represents for a spherical catenary the *directrix plane*, such that the length of chain hanging down to it gives the appropriate tension at any point.

The spherical catenaries may be replaced by continuous flexible cloth reaching up to the centre and thus representing an awning or velarium on a large scale, as the analysis required for these two distinct problems is the same.

A slight modification will serve for an umbrella with straight ribs; but the material as seen here retains the creases, and is not a very accurate representation. I should be grateful for a hint of some material or net work more suitable for experimental illustration; the steel-net-chain-work used for purses has been suggested but I fear it is manufactured in small pieces only.

It would add to the interest of the gyroscopic curves if the associated Poinsot herpolhode was drawn on the pavement. I have been asked whether it would be possible to give a mechanical link work representation of the motion. Darboux has shown us how a deformable hyperboloid can be employed to represent gyroscopic motion, provided a certain point is guided along this herpolhode; but the mechanical description of the herpolhode appears very complicated.

In The Physical Review some Photographic Traces of the Gyroscopic Pendulum have been given by Prof. Merritt. I hope he will take the numerical data of these stereoscopic views and see how far they may be reproduced photographically; friction will slightly perturb the form, but not to a very great extent it may be hoped.

Mr. Dewar made the numerical calculations and drew the diagrams. He takes an ordinary orthographic projection and lays of to scale the average distance between the eyes: the two stereoscopic figures are then the sections of the two cones of rays proceeding from the eyes; guiding points are set off by compasses from varying centres; the diagrams are drawn about three-fold size, and then reduced photographically on to the electrotype plate. Mr. Dewar has lost an eye, and so cannot judge of the solid effect, which is generally considered very perfect.

This is almost as pathetic as the case of Beethoven who, from his deafness, never heard some of his grandest compositions.

THE MAGNETIC SURVEY OF MARYLAND (FIRST COMMUNICATION). By Dr. L. A. BAUER, University of Cincinnati, Cincinnati, O.

DURING the summer of 1896 the writer was authorized by the State Geologist of Maryland, Prof. William Bullock Clark, to conduct a detailed Magnetic Survey of Maryland. About forty-six stations were occupied between September and December, 1896, the three magnetic elements—declination, inclination and horizontal intensity—being generally observed.

In addition to the funds provided by the State Geologist, the Association made a grant of $50 to assist in carrying on investigations in terrestrial magnetism in connection with the Magnetic Survey. The instrumental equipment was loaned to the State by the U. S. Coast and Geodetic Survey. The present paper will give some of the results of the 1896 work.

Owing to the interest manifested throughout the State in the Survey, and owing to the practical bearing of the work to the land surveyor, the Commission in charge of the State Geological Survey, decided to continue the Survey in 1897, special stress to be laid on the investigation of the local disturbances revealed by the 1896 work in Central Maryland. Some of the results from this recent work will likewise be touched upon.

[This paper is printed in Vol. I, Part V, Maryland Geological Survey.]

A NEW METHOD OF DETERMINING THE SPECIFIC HEAT OF LIQUIDS. By ROBERT L. LITCH, Bethlehem, Pa.

THE method has for its basis two special features :(1) obtaining a known amount of heat by means of the electric current, (2) maintaining the calorimeter cup at a constant temperature during the experiment.

These two features are introduced in the following manner. A test-tube of glass, with a coil of platinum wire inside, is set into the bulb of an air thermometer. Into this tube is placed a known amount of the liquid whose specific heat is desired. Its temperature is that of the room. Liquid of the same sort is cooled in a suitable apparatus to a temperature as near 0°C. as possible. A current of known strength is passed through the coil in the test-tube and the heat thus generated is neutralized by dropping a sufficient amount of liquid from the cooler. The calorimeter cup is thus maintained at a constant temperature and the corrections for "water equivalent" and "radiation" are avoided.

The simple formula

$$s = \frac{c^2 r t}{J m (T - T_o)}$$

may then be used for calculating the specific heat where

s = specific heat
c = current strength
r = resistance of coil
t = time of experiment (in seconds)
J = mechanical equivalent
m = mass of liquid dropped
T = room temperature
T_o = temperature of liquid dropped

If two similar calorimeters with their coils joined in series are used, the formula becomes

$$\frac{s_1}{s_2} = \frac{r_1}{r_2} \cdot \frac{m_2}{m_1} \cdot \frac{(T - T_{o_2})}{(T - T_{o_1})}$$

where the subscripts refer to the two calorimeters respectively.

This method is advantageous in that no knowledge is required of c, J or t.

By the first method in the case of water the following results have been obtained.

T	s
18.8°	.98075
19.71	.98064
21.05	.98035
21.2	.98035

[This paper will be printed in The Physical Review.]

EXHIBITION OF INSTRUMENTS FOR DETERMINING THE FREQUENCY OF AN ALTERNATING CURRENT. By Prof. GEORGE S. MOLER and Dr. FREDERICK BEDELL, Cornell University, Ithaca, N. Y.

THE instruments here included are two in number. The first one, which has already been described,[1] consists of a small synchronous motor brought to speed by a crank handle connected with the motor by a suitable train of gears. The apparatus contains an electrically operated speed counter, so arranged that its reading gives the exact value of the frequency of the alternating current with which the synchronous motor is supplied. The whole apparatus does not weigh over nine pounds. The reading is correct to within .05 of an alternation.

The second instrument consists of a sonometer or monochord. The alternating current flows through a piano wire mounted upon a sounding board. The wire passes between the poles of a permanent magnet. By means of a sliding bridge the period of the wire may be made equal to that of the alternating current. This is indicated by the vibration of the wire. A scale is arranged so that the position of the bridge indicates the frequency directly.

THE EFFECT OF PRESSURE ON THE WAVE-LENGTHS OF LINES OF THE EMISSION SPECTRA OF ELEMENTS. By Dr. W. J. HUMPHREYS, Johns Hopkins University, Baltimore, Md.

INCREASE of pressure about an electric arc increases the wave-lengths of the spectral lines so produced. This increase is different for different elements, and also for different series of lines of the same element. It is proportional to the wave-length of the line itself, and is a periodic function of atomic weight and consequently may be compared directly with any other property of the elements which itself is a periodic function of their atomic weights.

A NEW FORM OF COAL CALORIMETER. By CHARLES L. NORTON, Massachusetts Institute of Technology, Boston, Mass.

[*Communicated by the Secretary.*]

THE apparatus devised by the writer, and developed under his direction by students as thesis work, is intended to provide a means by which an engineer as well as a physicist may determine the heat of combustion of

[1] See "A Synchronous Motor for Determining the Frequency of an Alternating Current," by George S. Moler, Physical Review, March-April, 1897.

a fuel to an accuracy of one-half of one per cent. Owing to difficulty of manipulation, the forms of apparatus which require the use of oxygen under high pressure are not suitable for this use. The type of apparatus which uses oxygen under a pressure about that of the atmosphere has been carefully studied and alterations have been made to assure completeness of combustion, certainty of ignition and freedom from accidental losses of heat. The results of these changes are embodied in the designs for a new form of calorimeter.

[This paper will be printed in the Electrical Engineer, and the Engineering Record.]

NOTES ON THE HISTORY OF MUSICAL PITCH IN THE UNITED STATES. By Prof. CHARLES R. CROSS, Massachusetts Institute of Technology, Boston, Mass.

AT the Boston meeting of the A. A. A. S. (1880), a paper was read by the author of the present one, containing the result of an extended series of measurements of such tuning-forks, reeds and other standards of pitch used in this country as were accessible. This was the earliest series of measurements of the kind made in the United States.

The present paper contains the results of several hundred ratings of tuning-forks giving the standard pitch used by various piano and organ makers. These were sent by the manufacturers to a committee of the American Piano and Organ Manufacturers' Association, and transmitted by the late Gov. L. K. Fuller, of that committee, to the writer, by whom they were rated. The final ratings have not hitherto been made public.

The greater portion of the forks were at the pitch C_4, but a number were A_3 forks. Most of them were of the size and pattern ordinarily used by tuners. They were rated by comparison with a Scheibler tonometer made by Koenig. In all cases a comparison was made with at least two of the tonometer forks, and usually with a greater number. In general, five comparisons were made with each fork. The figure given is the mean of all the comparisons.

A METHOD FOR THE HARMONIC ANALYSIS OF ALTERNATING CURRENTS. By Prof. FRANK A. LAWS, Massachusetts Institute of Technology, Boston, Mass.

[*Communicated by the Secretary.*]

THE method consists in determining the maximum mean product of the current to be analyzed and a sinusoidal current, 1, 2, 3, 4, etc., times its periodicity.

The integration is effected by an electro dynamometer, the current subject to analysis flowing in the stationary coil and the sinusoidal current in the movable coil.

Arrangements are made for determining the phase and the sign of the components.

THE DETERMINATION OF THE SURFACE TENSIONS OF WATER AND OF CERTAIN DILUTE AQUEOUS SOLUTIONS BY MEANS OF THE METHOD OF RIPPLES. By Dr. N. ERNEST DORSEY, John Hopkins University Baltimore, Md.

THE apparatus employed is a development of that used by Lord Rayleigh. The solution is contained in a porcelain developing tray, 2.5 by 29 by 36 centimeters, and the waves are generated by means of a plate of glass attached to the lower prong of a large tuning-fork. By the side of the tray is placed a dividing engine which is horizontal and parallel to the direction of propagation of the waves, and to whose carriage are attached two arms which project over the tray and which carry metallic mirrors at their further end; on the carriage is mounted a telescope also. By means of these mirrors a horizontal beam of parallel light, that passes above the surface of the solution and is parallel to the engine, is reflected to the surface and then back and into the telescope. This beam of light is rendered intermittent and isoperiodic with the waves by being passed through holes in two light blades of copper attached to the extremities of the prongs of another large fork whose vibrations are maintained electromagnetically and which is tuned to unison with the first. The fork that generates the waves is driven by means of a current shunted off from the circuit of the other fork. Now when the forks are vibrating and the telescope is focussed for the light reflected from the crests of the waves, its field will be crossed by a series of parallel bright lines which correspond to the crests. Hence the wave length can be easily measured with the dividing engine. The rigidity of the mirror mountings, the accuracy of the ways, and the effect of viscosity were tested experimentally and found to be such as to produce no effect on the wave length.

In the series of twenty-one determinations of the surface tension of water the average was 73.24 dynes per centimeter at 18°C., and the average deviation of a single observation from this mean was only one-fifth of one per cent. The concentrations of the solutions have been varied from one-tenth normal to normal, and in this region the surface tension is a linear function of the concentration, and the values found agree in most cases with those obtained by others.

[A full account of the work will be found in the Physical Review for September and October.]

THE SERIES OF INTERNATIONAL CLOUD OBSERVATIONS MADE BY THE U. S. WEATHER BUREAU, AND THEIR RELATIONS TO METEOROLOGICAL PROBLEMS. By Prof. FRANK H. BIGELOW, U. S. Weather Bureau, Washington, D. C.

A BRIEF statement of the methods of observing the heights, the direction and velocity of motion of clouds, at the Washington primary station with alt-azimuth instruments, and at fourteen secondary stations with nephoscopes, was made as an introduction to the main portion of the subject. Then an account was given of the construction of the general equations of motion applicable to the atmosphere over a hemisphere of the earth, and the resulting circulation of the air, with cold polar and warm tropical regions, as deduced by Ferrel. The equations of motion for local circulation, such as occurs in cyclones and anti-cyclones, as deduced by Ferrel, Sprung, Guldberg and Mohn, Oberbeck, and by Pockels, in their respective papers, were reviewed, showing the assumptions laid at the basis of their solution of the differential equations. A series of criticisms followed tending to prove that these solutions are *ideal* rather than true representatives of the natural phenomena, and that the popular theory of storms, deriving them from the convection of a vertical central current and the latent heat of precipitation, is very faulty. For (1) the equations imply a conservative circulation using the same air over and over again, but the cloud movements indicate a streaming of fresh material in a cyclical circulation through a fixed configuration of isobars; (2) the warm centre is not justified because the isothermals in West Indies hurricanes are not circular about a centre but pass nearly straight across, and in the cyclones of the United States they are merely deflected by counter currents of cold and warm air impinging along the axis of the cyclone; (3) the latent heat theory is not necessary because dry cyclones, with deep depressions of the isobars, originate in Alberta and traverse the Lake Region to the Gulf of St. Lawrence, lasting several days, moving 3000 miles and yet with little rainfall. The cloud observations indicate that the active part of storms is confined to the lower strata of the atmosphere within three miles of the ground, that Ferrel's general circulation holds true, except for slight deflections, above the 3-mile level; that the lower strata are disturbed by the existing temperature gradients over the oceans and continents, which tend to reduce the general stream lines to local vortices; and that the speed of the eastward drift in mid latitudes is slowed down by local breaks in the general gradients, this also contributing to form cyclonic vortices. The author expressed the opinion, that defective general gradients, and the counter flows from adjacent highs, are chiefly concerned in producing lows, and that the latter are strictly dependent upon the general circulation for their power, rather than upon any local overheating effects, such as Ferrel sought to rest upon in his discussion of cyclones. Tornadoes and small vortices may, perhaps, be the only instances of true cyclones with warm centres in the atmosphere. The paper was illustrated with a number of diagrams.

KITES AND THEIR USE BY THE WEATHER BUREAU IN EXPLORATIONS OF THE UPPER AIR. By C. F. MARVIN, Professor of Meteorology, U. S. Weather Bureau.

1. BRIEF historical mention of the use of kites as a piece of scientific apparatus.

2 The investigations to determine the availability of kites in the daily work of the Weather Bureau as inaugurated by Prof. Willis L. Moore: first, the development of mechanically sound and scientific methods of flying kites in the most effective manner; second, improving and perfecting the form and body of the kite; and, lastly, the invention of automatic meteorological instruments adapted to be borne aloft by the kites and secure accurate and trustworthy records of the velocity and direction of the wind, the temperature, pressure, and humidity of the air, etc.

3. General statement of the mechanical laws of the flight of kites and explanation of their behavior.

4. Circumstances under which the most efficient action results.

5. The gratifying nature of the observations thus far obtained and their value in respect to the forecast work of the Weather Bureau.

6. The deep interest in the investigations with kites shown by the Honorable James Wilson, Secretary of Agriculture, and the steps being taken to extend the work so that daily charts of the atmospheric conditions may be made, not only at the surface, but upon an imaginary plane in the free air at an elevation of from one to two miles or more.

7. Concluding remarks.

THE EFFECTS OF TENSION AND QUALITY OF THE METAL UPON THE CHANGES IN LENGTH PRODUCED IN IRON WIRES BY MAGNETIZATION. By Dr. B. B. BRACKETT, Union College, Schenectady, N. Y.

ALL the specimens of iron wire experimented upon were about 2 m. long and 1.25 mm. in diameter. While under test a wire was suspended vertically so that tension could be applied by weights attached to its lower end. The variations in the length of 70 cm. of the wire within the solenoid was then observed by means of mechanism, employing a lever and tilting mirror, in such a way as to magnify the actual changes about 25,000 times, and hence to give readings for $\frac{dl}{l}$ multiplied by 1,750,000.

The changes in length caused by a definite magnetizing field were read both with the field on and after it had been removed.

Results obtained:—

1. NATURAL PIANO-WIRE.

No change of length was observed until the fields were reached at which the magnetization rises rapidly. Changes in length were first observed

when the field was removed. This was an elongation and the length of the wire with the field off continued to increase up to the field strengths at which the induction curve turns towards the horizontal. From that point on, the wires were permanently elongated with the field off. But this value of $\frac{dl}{l}$ did not exceed 3.5×10^{-7}. At a very short distance beyond the point where the wire begins to lengthen as the current is broken, it shows its first change of length with the field on, by beginning to contract. From this point on to the end, the wire contracts with the field on and the contraction increases apparently in direct proportion to the field until at the end, for a field of 300, $\frac{dl}{l}$ equals about 30×10^{-7}.

The curves for the different tensions differ but little.

2. ANNEALED PIANO-WIRE.

This showed elongations with the fields both on and off up to the point where the induction curve turns toward the horizontal. The elongations here are greater with the field off than when it is on. Not far from the turning point of the induction curve, the elongations with the field off cease to show any change, this value of $\frac{dl}{l}$ not exceeding 7×10^{-7}. At about the same point the curve with field on begins to descend along a nearly straight line, just as it did before the annealing. But here the contraction is more rapid, the slope for the straight part of the curves, as plotted to field, being nearly 50 per cent greater than before annealing.

Increased tension does not affect the general shape of the curves. But it diminishes the amount of the elongation and increases the final contraction with field on, while it causes both elongation and contraction to begin at weaker fields.

3. SOFT ANNEALED WIRE.

The curves for very soft wire are in general exactly like those for the annealed piano-wire, except that the changes of length are very much greater, and in some cases for weak fields and slight tension, the wire shortens when the field is removed But finally the elongation with the field off becomes as great as or even greater than the maximum elongation with the field on. After this the length tends to remain constant until the end of the experiment. This permanent lengthening for the soft iron gave a value of $\frac{dl}{l}$ as great as 33×10^{-7} for the least tensions used. The straight portion of the curves with field on shows a contraction that is much more rapid than for annealed piano-wire

Increased tension showed exactly the same effects as upon the annealed piano-wire, though the amount of the changes due to tension was much greater.

CONCLUSIONS.

All the phenomena observed can apparently be explained by the following:—

1. An *elongation* depending upon the degree of magnetization.
2. A *contraction* proportional to the magnetizing field.
3. An *increase in Young's Modulus* also dependent upon the degree of magnetization.

[This paper will be printed in The Physical Review.]

THE MEASUREMENT OF SMALL GASEOUS PRESSURES. By CHARLES F. BRUSH, Cleveland, Ohio.

PRIOR to the invention of the McLeod vacuum gauge, the measurement of even moderately small gaseous pressures was difficult, and subject to large errors. The introduction of the McLeod gauge, however, seemed to solve the problem. In its ordinary form, the chief cause of difficulty is the unequal and variable capillary depression of the two small columns of mercury, whose difference in height indirectly serves as the measure of pressure.

For an accurate means for measuring small pressures I constructed the modified form of McLeod gauge, which it is the purpose of this paper to discuss.

The diagram, herewith, shows the essential parts of my apparatus. The bulb A, of the gauge, is made conical in its upper part to avoid adhesion of gas bubbles when the mercury rises. This bulb holds about eleven pounds of mercury. B and C are the gauge head and comparison tube respectively. They are nearly twenty millimeters inside diameter, and are made from contiguous parts of the same carefully selected tube. D is the usual air trap, and E is a long glass tube, with flexible pure rubber connections to the lower end of the gauge stem, and the mercury cistern F. The latter is mounted on a carriage G, which moves vertically on fixed guides. The height of the carriage is adjustable, at the upper end of its range of motion, by means of the screw H, thumb-nut I, and forked support K. The screw is pivoted to the carriage, so that it may swing out of the fork when the carriage is lowered. L is a pinch-cock with screw, for regulating the flow of mercury, or stopping it altogether, while pumping out the trap D. N is a bulb containing phosphorus pentoxide, to keep the interior of the gauge and other parts of the apparatus perfectly dry. P is a cathetometer for observing the mercury columns in B and C. It has a revolving column with vertical scale, and vernier with microscope, reading to hundredths of a millimeter. The eye-piece micrometer reads directly to hundredths of a millimeter, and the divisions on the revolving head of the screw are so open, that tenths of divisions are easily and certainly estimated by an experienced eye; thus permitting the micrometer to be read directly to thousandths of a millimeter. Of course the cathetometer is permanently located not as shown, but with the objective of its telescope equally distant from the

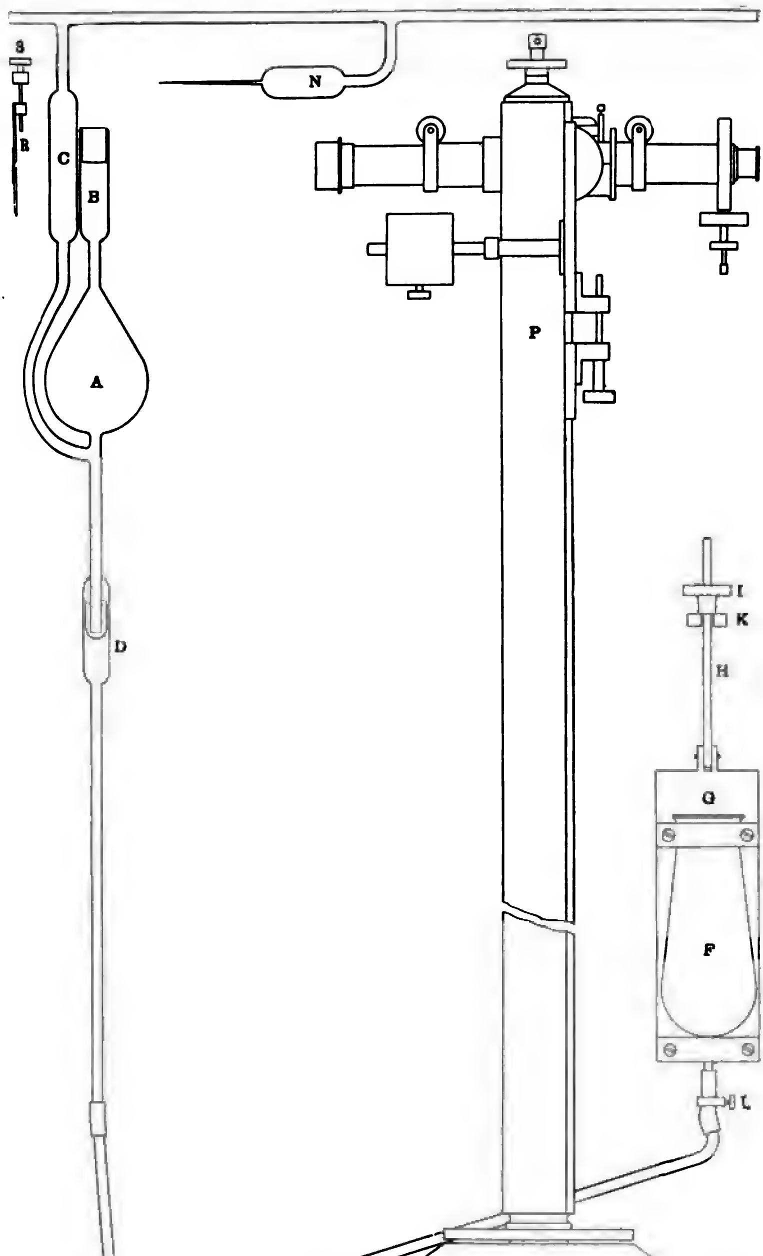
axes of the tubes B and C, when it is alternately directed to them; and at such a distance that its micrometer readings correspond to a millimeter scale.

The most important part of the gauge is the head B. The purpose of its great diameter is the reduction of capillary depression in its mercury column. But its size necessitates a very close approach of the mercury to its upper end, in order sufficiently to reduce its capacity. Yet the remaining space must be measurable by the cathetometer, with the utmost precision. Hence the glass must not be distorted by heating, and the closed end just over the mercury must be sharply defined. In constructing this part of the apparatus, I selected a piece of heavy tubing which would just slip inside of B, with the least possible clearance. One end of this tube was closed as squarely as possible by fusion, and then ground, with fine emery and a suitable tool, to a convex spherical surface of long radius. Care was taken to make the center of curvature lie in the axis of the tube, and the ground surface was left unpolished to facilitate observation. A suitable length of the closed end of the tube was then cut off, slipped into B, and both tubes were fused together at their open ends, as shown.

For calibrating the head B, a ground glass stopper with a capillary duct was fitted to its neck, before the latter was sealed to the bulb A. The head was then filled with mercury by boiling, thus completely filling the small space between its wall and the cap. After cooling, the stopper was inserted to expel all excess of mercury, and the whole weighed. Next the head was emptied, and the mercury in the annular space distilled out. Again the head was very nearly filled with mercury, without allowing any to get into the annular space, and weighed as before; and the space between the top of the mercury and the convex end of the head was very carefully measured by the cathetometer. This process of weighing and measuring was repeated several times, with less mercury each time. Thus the capacity of a vertical millimeter of the head was ascertained, as well as the capacity that would remain, if the top of the meniscus of mercury just touched the convex end of the gauge, above it. Finally the neck was sealed to the bulb *A* and the capacity of head, neck and bulb combined, was found by weighing them empty, and again filled with mercury.

For lighting the top of each mercury column, a narrow horizontal slit, in an opaque screen R, is used. The slit is covered with a strip of ground glass, and obliquely illuminated by an electric lamp. The screen and slit are vertically adjusted by a thumb screw S. The heat of the lamp is prevented from reaching the mercury columns, and head B, by a thick screen. This is very necessary.

In order to get the best results from the apparatus, many precautions are necessary. After filling A and B with mercury, time must be allowed for the compressed gas to cool. The effect of changing barometric pressure is nearly eliminated by so regulating the quantity of mercury in F, that its surface is in the small tube at the bottom of the cistern, when the gauge is properly filled. Its area is then very small, as compared with


S
R
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that in B and C. The height of the meniscus in both tubes is easily adjusted sensibly equal, by a little manipulation. I *always* raise the mercury above the point at which readings are to be taken, and then lower it, so as to read on a falling meniscus. This is highly important.

Some trouble was occasionally experienced at first, from electro-static induction between the mercury in B, and the glass above it. This was shown by distortion of the meniscus when it was brought very near the glass. The difficulty was partially, but not wholly remedied, by putting mercury in the outside open end of the gauge head, and connecting it by a flexible conductor with the mercury in the cistern F. A complete remedy was effected, by moistening the inside of the gauge head with a dilute solution of phosphorus pentoxide. This became completely dried by the anhydrous phosphorus pentoxide in N, but was of course not dehydrated; and hence always remains conducting, and dissipates static charge.

Large pressures, up to a thousand millionths or more, are readily measured with this apparatus, by finding with the cathetometer the distance between the mercury in B, and the end of the head above it: from this is quickly calculated the necessary multiplier for the number of millimeters difference in height between the columns in B and C, also measured by the cathetometer, in order to express the result in millionths. For very small pressures, the micrometer wires are set at such a distance apart, as to give a convenient constant (usually 2); and the column in B is adjusted this distance away from the glass; careful allowance being made for the thickness of the wires. Then the micrometer is used for repeated measurements of the difference in height of the mercury in B and C. The disturbing effect of *bias* is entirely eliminated by giving the micrometer screw a partial turn after each reading.

In a sample set of thirty readings, the calculated probable error of the thirty readings taken together was less than a thousandth part of a millionth of atmospheric pressure. The probable error of the three mean results, considered as single readings, is only eleven hundredths of a unit in the third decimal place of millionths. The net result may be expressed as follows, in terms of atmospheric pressure: considered as thirty measurements, $0.000\ 000\ 434\ 60 \pm 0.000\ 000\ 000\ 92$; considered as three measurements, $0.000\,000\ 434\ 60 \pm 0.000\ 000\ 000\ 11$. Here we have the measurement of a total quantity of less than half a millionth of atmospheric pressure, with a probable error of only about a fifth of one per cent of the quantity measured.

In another example, we have the measurement of about two millionths of atmospheric pressure, with a probable error of only one part in three thousand, of the quantity measured. From the foregoing, we may safely conclude that, with the apparatus described, small gaseous pressures may be easily measured, with a probable error of less than a thousandth part of a millionth of atmospheric pressure.

[This paper is printed in the Philosophical Magazine.]

ON THE COËFFICIENT OF EXPANSION OF CERTAIN GASES. By Prof. EDWARD W. MORLEY and Prof. DAYTON C. MILLER, Cleveland, Ohio.

THE coëfficients of expansion of hydrogen, nitrogen and carbon dioxide have been determined by the International Bureau of Weights and Measures with great precision, the experiments having been undertaken in connection with investigations on the air-thermometer. The coëfficients of a few other gases have been determined with much less precision, while those of many gases have not been determined at all. The method used in the experiments here described is a differential one in which the expansion of the unknown gas is compared with that of hydrogen.

Two glass globes of five liters capacity are to be either packed in ice or subjected to a steam bath. These globes can be connected by glass joints to the air pumps and the gas generators, or can be sealed by fusion at pleasure. They are also connected by symmetrical capillary glass tubes, to a differential mercury manometer, having mercury columns twenty-five millimeters in diameter. By means of a suitable cathetometer and illuminating device, the difference in the heights of the mercury columns can be read with certainty, to less than the hundredth of a millimeter.

Both globes were exhausted and filled with hydrogen to seventy-six centimeters pressure, and subjected to the ice and steam baths alternately, to enable us to study the behavior of the globes themselves. Then the hydrogen was removed from one globe and another gas was introduced at very nearly the same pressure. By again surrounding the globes with ice and steam alternately, the expansion of this unknown gas as compared with that of the hydrogen, which was in this *same globe* under similar condition, was determined by comparisons with the hydrogen in the "comparing globe," which is not disturbed during the whole series of experiments.

Using this method three series of measurements were made of the expansions of oxygen, nitrogen, carbon dioxide and air. Other gases are to be experimented upon in the future. The results so far obtained are, the coëfficient of hydrogen being assumed as 0.0036625, that of carbondioxide is 0.003712, of oxygen 0.003673, of nitrogen 0.003672, of air 0.003672.

NOTE ON THE CONSTRUCTION OF A SENSITIVE RADIOMETER. By Prof. ERNEST FOX NICHOLS, Colgate University, Hamilton, N. Y.

IN this paper the construction of a new form of compensating torsion radiometer, used recently in a number of researches in the remote infra-red spectrum, was described in detail. The instrument is capable of a higher degree of sensitiveness than either the spectro-bolometer or linear

thermopile. A steadiness of action and freedom from extraneous disturbances, when working at high sensitiveness. are secured by means of the compensating action of two precisely equal vanes symmetrically mounted on either side of the axis of a quartz fiber suspension. The system, in consequence, is acted upon differentially by all accidental disturbances, while rays to be measured are concentrated upon one of the vanes. The degree of sensitiveness actually attained in one instance was so great that the influence of rays from a single candle, at a distance of one-third of a mile, could be detected and roughly measured.

THE PHOTOGRAPHY OF MANOMETRIC FLAMES. By Prof. EDWARD L. NICHOLS and Prof. ERNEST MERRITT, Cornell University, Ithaca, N. Y.

THE manometric flame used in this investigation was similar to that employed by Merritt in 1893[1]. Acetylene gas was, however, substituted for the enriched coal gas used in the former experiments. This gave a flame of great actinic brightness. Instead of a glass plate shot past a slit in the camera, celluloid films were used. These were mounted upon a drum 110 cm. in circumference. The drum, which was driven by means of an electric motor, was given a nearly uniform speed of about 1 m. per second. The photographs obtained were used in the study of the following points:

First, for the comparison of the motion of the flame produced by initial consonants, followed by various vowels, with the movements produced by the same consonant when occurring in the middle of a word or at the end.

Secondly, to determine in how far small differences of speech show themselves in the flame photographs.

Thirdly, to bring out the gradual change in quality of vowel sounds as the organs of speech are modified in articulation. It was found that the records produced by consonants were very insignificant as compared with those produced by vowel sounds, and that in many cases it was difficult to distinguish between the cessation of motion of the flame during the enunciation of a consonant in the middle of a word and the pause occurring between successive words in a spoken sentence. The records obtained were, however, sufficiently definite to indicate that this method might be of great use in the graphical study of phonetics. Not only are differences of dialect easily distinguishable in the records, but likewise differences in the individual voices of those speaking the same dialect.

[1] Physical Review, Vol. 1, p. 166.

ON THE ELECTROSTATIC CAPACITY OF A TWO-WIRE CABLE. By Prof. G. W. PATTERSON, JR., University of Michigan, Ann Arbor, Mich.

(Read in Joint-Session with Section A.)

"THE electrostatic capacity of a cable made of two equal wires is

$$\frac{0.01206\ K}{\log.\frac{\sqrt{4Rd+d^2}+d}{\sqrt{4Rd+d^2}-d}}$$

microfarads per kilometer, when K is the specific inductive capacity of the dielectric, R the radius of each conductor, and d the least distance between them and common logarithms are to be used."

[This paper will be printed in the Physical Review.]

THE TREATMENT OF DIFFERENTIAL EQUATIONS BY APPROXIMATE METHODS. By Prof. W. F. DURAND, Ithaca, N. Y.

(Read in Joint-Session with Section A.)

[FOR ABSTRACT, SEE PROCEEDINGS SECTION A.]

A NEW METHOD OF SOLVING CERTAIN DIFFERENTIAL EQUATIONS THAT OCCUR IN MATHEMATICAL PHYSICS. By Prof. ALEXANDER MACFARLANE, Lehigh University, So. Bethlehem, Pa.

(Read in Joint-Session with Section A.)

[FOR ABSTRACT, SEE PROCEEDINGS SECTION A.]

ON THE RATE AT WHICH HOT GLASS ABSORBS SUPERHEATED WATER. By Prof. CARL BARUS, Brown University, Providence, R. I.

IF water under pressure is heated in glass tubes, the volume of water contained decreases as the square, whereas, the chemically active area decreases as the first power of the diameter. In proportion as the tube is more capillary, the action of the water on the glass produces accentuated volume effects. Thus it was shown that the confined volumes of glass and included water undergo *contraction* at 180°, forming an eventually solid silicate, while compressibility increases to fully three times its value at

the beginning of the experiment. The measurements further show that about .025 cm. of liquid water is absorbed per square cm. of glass surface, at 180°C. per hour. The effect of absorption in the tubes used, amounted to 18% of the total bulk of water per hour. The author points out the bearing of this large and rapid contraction in vulcanology

[To be printed in full in Philosophical Magazine, London.]

THE PREDETERMINATION OF THE REGULATION OF A TRANSFORMER WITH NON-INDUCTIVE LOAD. By DR. FREDERICK BEDELL, Prof. R. E. CHANDLER and Mr. R. H. SHERWOOD, Jr.

To determine the regulation by this method a wattmeter, ammeter and voltmeter are located in the primary circuit. One set of readings is taken with the secondary short-circuited by a stout copper wire, the primary voltage being adjusted until the normal full-load current flows in the transformer or any desired fraction of it. No other data for obtaining a complete regulation curve is required except the one set of readings above mentioned and the magnetizing current. If a wattmeter reading is taken when the magnetizing current is measured, the data is sufficient to plot a complete efficiency curve as well as a curve for the regulation of the transformer. The two sets of measurements then consist of the reading of a wattmeter, voltmeter and ammeter, first on short-circuit with normal current, and second on open-circuit at normal voltage. The wattmeter reading in the first case gives the copper losses; in the second case, the core losses. It is commonly convenient to use the high-potential coil as primary in the short-circuit measurements, and the low potential coil as primary in the open-circuit measurements. A high potential supply is not then needed, and as no power is required except to supply the losses, the complete test of a transformer may be made with an incandescent lighting circuit for the source of supply, a 50 light transformer being tested from one 16 c p. lamp socket. The total drop is found by laying off in the proper manner the inductive drop, the magnetic leakage drop and the drop due to ohmic resistance. The method is theoretically an almost exact one. Practically it is an exact method and less likely to error than the ordinary method of determining the regulation of a transformer by loading it. The results given in this paper (given in full in The Electrical World), from a long series of tests on seven transformers of various makes, show the reliability of the method, the secondary voltage at full load determined by it varying usually less than one or two tenths of a volt from the voltage as found by measurement on the transformer when actually loaded. An approximate method (given by Kapp) used by one of our large electrical companies gives less accurate results. The reader is referred to the data given in the full paper.

[This paper printed in the Electrical World, Aug. 14, 1897.]

A METHOD OF OBTAINING CAPILLARY PORES OF SPECIFIED DIAMETER By Prof. CARL BARUS, Brown University, Providence, R. I.

IN a previous paper the author showed how to compute the mean value of the pores of a porous septum by the pressure difference just necessary to force air through the wet barrier against the capillary reaction of the water. In the present paper capillary pores of larger diameter are obtained by puncturing elastic rubber tubing with 5,000 to 10,000 holes to the linear meter. The mean size of the pores is again computed from the pressure difference just sufficient to cause a gaseous flow through the wetted elastic septum, and this size may be decreased at pleasure by thickening the walls of the tube. The author then describes a series of striking experiments to be made with these tubes. Among others the flow of water through the pores if observed during increase and during decrease of pressure, respectively, is a cyclic phenomenon.

[To be printed in full in The Physical Review.]

AN ELECTRICAL THERMOSTAT. By Dr. W. R. WHITNEY, Jamestown, New York.

THE essentially new appliance, the regulator, depends for its efficiency upon the difference in the effects of a temperature change on the pressure of a saturated vapor and on that of a gas. This regulator is practically a U tube partly filled with mercury, over which in one arm is confined a saturated vapor, as ether, and in the other, air confined under a pressure depending upon the temperature desired. Two wires enter through the walls of the tube, one at its base and the other about half way up one arm, so that, when in adjustment, the contact between the latter wire and the mercury is made or broken by the slightest change of temperature. A current from three Daniell elements, in series, passes through these wires and through the coils of a telegraph sounder. This sounder makes and breaks the heating current which is taken from the ordinary lighting circuit. The heating coil consists of fine, naked platinum wire coiled upon glass in the bottom of the bath which contains the regulator, and has a resistance of about 100 ohms. The temperature of the baths at 25° C. has been kept a week with a maximum deviation of but a few thousandths of a degree.

A NEW FORM OF RESPIRATION CALORIMETER AND ITS USE FOR VERIFYING THE LAW OF THE CONSERVATION OF ENERGY IN THE HUMAN BODY. By Prof. W. O. ATWATER and Prof E. B. ROSA, Wesleyan University, Middletown, Ct.

THE apparatus was developed at Wesleyan University during the years 1892-7, in connection with an investigation on the Metabolism of Matter and Energy in the human body. The whole investigation has been con-

ducted under the patronage of the Storrs Experiment Station of Connecticut, the United States Department of Agriculture, and Wesleyan University.

The work consisted in determining, on the one hand, the total amount of energy taken into the body of the person under test, which is ascertained by burning in a suitable calorimeter samples of the food eaten and finding their heat equivalent. The foods are analyzed (by sample) and the amount of C, H, N, etc., determined. Temperatures of food and drink are also taken. The unabsorbed energy expelled from the body in the excreta is also determined, together with the chemical composition of the same.

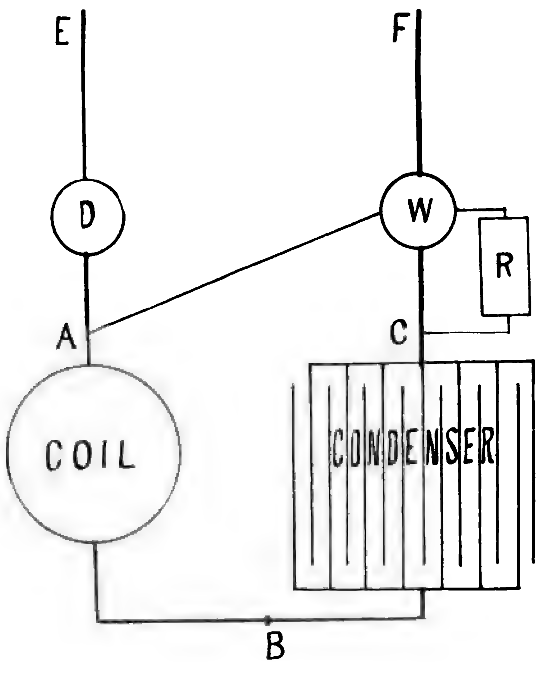
The total amount of heat given off from the body is determined by placing the person under test in a large respiration calorimeter, and accurately measuring this heat The calorimeter was seven feet long, six feet four inches high, and four feet wide, internal dimensions A person could eat, sleep and work in this chamber, and in practice was sealed up and observations continued for from two to twelve days at a time. A current of pure air circulated through the chamber constantly, the CO_2 and H_2O contained in it as it entered and again as it left the chamber being measured. A stream of cold water flowing through a copper pipe carried away the heat as rapidly as it was generated, and so kept the temperature of the chamber constant. The chamber had a double wall of sheet metal, and by suitable devices, including a system of thermoelectric junctions, the temperature of the two walls was kept exactly the same, so that no heat was gained or lost by flowing through the walls of the chamber.

Work done by the person under test was measured, and the total energy absorbed from food was balanced against the heat and work yielded. The apparatus is necessarily complicated, and the experiments laborious and time-consuming. The results are, however, exceedingly gratifying, and will be published in full by the United States Department of Agriculture.

ELECTRICAL RESONANCE AND DIELECTRIC HYSTERESIS. By EDWARD B. ROSA and ARTHUR W. SMITH, Wesleyan University, Middletown, Ct.

THE amount of energy absorbed by the dielectric of a condenser is usually quite small, and difficult to measure. The difference in phase between the current and impressed electromotive force is in the neighborhood of 89°, and hence the power factor ($\cos \theta$) in the expression $W = E I \cos \theta$ is very small. If the current in the shunt circuit of a wattmeter lags half of one degree on account of self induction (as is entirely possible), the reading of the wattmeter used to measure the loss of energy in the condenser would thereby be reduced 50 per cent. A further difficulty arises from the fact that because the power factor is so small the shunt current must be very large in order to get a deflection large enough to read accurately. This large current from a high voltage supply heats up the shunt coils very greatly, and altogether the difficulties are too great to be easily overcome.

In order to bring the current and electromotive force into phase with each other, and at the same time to reduce the voltage to a low value, and thus make the problem of measuring the power with a wattmeter the simplest possible one, we have placed a large coil of copper wire in series with the condenser, the coil having sufficient induction to give perfect resonance, and the current through coil and condenser therefore being in the same phase. The connections are shown in the figure. E F are the terminals of a dynamo or transformer, giving a low electromotive force, say 50 volts. D is a dynamometer, W a wattmeter, and R is a non-inductive resistance, in series with the movable coil of the wattmeter. A and C are the terminals of the shunt circuit. The wattmeter therefore measures the



energy expended on the coil and condenser and connecting wires. With an ammeter and voltmeter the resistance from A to B is measured, and the C^2R loss is subtracted from the total energy measured by the wattmeter. The difference is the energy spent on the condenser. The resistance of the coil was only .3 ohm, and the energy spent on the coil was only about one-third of the total. In order to get a sufficiently large induction without greater resistance a very large coil is necessary, the one used weighing 400 pounds. It was kindly loaned for this work by the American Electrical Works, of Providence, R. I.

The resonance was so good that with 49 volts impressed upon coil and condenser, the voltage between the terminals of the coil alone or of the condenser alone was 2250 volts, a multiplication of 46 times.

Experiment shows that the loss in the condenser is almost exactly proportional to the square of the difference of potential of the plates of the condenser. The latter was made by the Stanley Electric Co., and the tin foil is separated by sheet of paper filled with the solid dielectric. It showed a remarkable dielectric strength, and comparatively small hysteresis loss. The loss varied from 1.0% to 1.5%, according to the frequency employed, the efficiency being greater at lower frequency.

These experiments are preliminary to others soon to be made, the results of which will be published in detail.

A METHOD FOR THE DETERMINATION OF THE PERIOD OF ELECTRICAL OSCILLATIONS AND OTHER APPLICATIONS OF THE SAME. By MARGARET E. MALTBY, Ph.D., Painesville, Ohio.

THE method depends upon the balancing of a resistance against a capacity, the one joining the needle and one pair of quadrants in a modified quadrant electrometer. the other joining the needle and the other pair of quadrants. Electrical oscillations that excite the system are a simple sine function of the time. The solution of the equations of condition in this arrangement results in a simple expression for the period T in terms of the capacity C, and the resistance R, viz., $T = \pi R C$.

This relation was established experimentally by sending through the system a current excited by the oscillatory discharge of a condenser of known capacity $\mathfrak{C}$, through a sparkgap and a conductor of known coëfficient of self-induction L. The period of these oscillations is given by the formula of Lord Kelvin, $T = \pi \sqrt{L \mathfrak{C}}$. The periods determined by these two methods show the agreement of the same within the limits of the estimated precision.

If the period of the alternating current is known, this arrangement of capacity and resistance affords a method for determining v, the relation between the electrostatic and electromagnetic system of units. Let R be measured in the latter and C in the former system, then the homogeneous equation becomes $T = \pi \frac{R C}{v^2}$, or $v = \sqrt{\frac{\pi R C}{T}}$. The results confirm the applicability of the method.

Further, coëfficients of self-induction can be measured in this way. In place of either the resistance or the capacity connected with the electrometer substitute in succession a capacity C, a resistance R_1, whose coëfficient of self-induction L is to be determined, and a resistance R_2 without

self-induction, equilibrium being established in each case with the same resistance or capacity in the other arm of the bridge, and the period of the oscillations remaining constant. From the equations of condition in this case, $L = CR_2 \sqrt{R_2^2 - R_1^2}$.

With the same general procedure the dielectric constant of an electrolyte can be determined. In this case substitute successively a condenser of capacity C without galvanic conductivity, a resistance R_1 without sensible self-induction and dielectric conductivity, and an electrolytic trough whose galvanic resistance is R_2, specific conductivity k and dielectric constant D. Thus it follows from these conditions that,

$$D = 4\pi Ck \sqrt{R_2^2 - R_1^2}.$$

For the last two applications only preliminary experiments have been made thus far.

The convenience and wide applicability of the method recommend it.

[Printed in part in Wiedemann's Annalen d. Physik u. Chemie, bd. 61 (1897) s. 553.]

On methods of measuring mean horizontal candle power. By Prof. C. P. Matthews, Purdue University, Lafayette, Indiana.

[*Communicated by Prof. E. L. Nichols.*]

The object of these measurements is to compare the method of obtaining the mean horizontal candle power in a glow lamp by whirling the lamp about a vertical axis or symmetry, at a speed sufficient to eliminate flickering, with the usual method of integrating the curve of horizontal distribution as taken point by point. The former method has been recommended by a committee of the American Institute of Electrical Engineers as the standard method of procedure. In order to subject the matter to a severe test an exceedingly irregular distribution was artificially obtained by pasting pieces of black paper on the bulb of an incandescent lamp. The lamp was whirled at different speeds, from a minimum which caused a barely perceptible flicker, to a maximum beyond which it was not considered safe to go. No variation in intensity with speed was found. The distribution curve for this lamp was taken at ten degree intervals. The curve was plotted in rectangular coördinates and was integrated by means of the planimeter. The mean horizontal candle power determined in this manner was 9.655; the mean of all the readings on the whirling lamp was 9.649. The difference amounting to .006 or about .06 per cent is entirely within the limits of photometric error. It is the intention of the author to extend this comparison to the case where there is a marked color difference between the sources under investigation.

[This paper will be printed in the Physical Review.]

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ADDRESS

BY

WILLIAM P. MASON,

VICE PRESIDENT, AND CHAIRMAN OF SECTION C.

EXPERT TESTIMONY.

IT will be remembered that a would-be facetious barrister once remarked that prevaricators might be properly arranged in an ascending series, to wit: ordinary fibbers, liars, and experts; an arrangement which I fear meets with the approval of many members of the bench and bar to-day. The cause for such harsh classification is not so very far to seek. It is based upon ignorance on the part of the bar, and at times upon what is worse than ignorance on the side of the "expert" With the culpable acts of the pseudo scientist we cannot waste our time. That he merits prompt condemnation is axiomatic; but a word is wanted touching upon what may be termed the ignorance of the court.

"When I take my place upon the witness stand," said a prominent toxicologist once to me, "I can never predict in what shape I shall be upon leaving it," a feeling with which most of us can, I fancy, sympathize pretty keenly.

Is it that we fear exposure of the weak points in our professional armor? Do we dread to say in public "I do not know?" Hardly that, I take it. We are now possessed of so very little of that which one day may be known, that no true scientist hesitates for an instant to plead legitimate ignorance. What really troubles us upon cross-examination is that the court does not speak our language, a language often quite difficult of direct translation; that it is but rarely schooled in the principles of our science; and that, in consequence, it frequently insists upon categorical answers to the most impossible kind of questions.

The hypothetical questions showered upon the expert witness are sometimes veritable curiosities, so peculiar are they in their monstrosity. Who among us but has felt that the layman, who has simply to testify to observed facts, has an easy time of it indeed, when compared with him from whom there is expected an opinion under oath?

All scientific men are willing and anxious to have their work scrutinized carefully by their peers; but to be exposed to the one-sided criticism frequently encountered at the bar is quite another matter; for it must be remembered that after the adverse counsel has opened up what appears to be a glaring inconsistency in the testimony, the re-direct examination may utterly fail to repair the breach, because of a lack of familiarity with a technical subject on the part of the friendly attorney.

This leaves the witness in the unenviable position of disagreeing with the general drift of his own testimony, while it deprives him of suitable means of insisting upon its revision and correction.

According to the writer's view, there is but one way to escape such dilemma, and that is by direct and immediate appeal to the judge; urging that the oath taken called for a statement of the whole truth, and not the misleading portion already elicited.

To illustrate how serious a matter the partial testimony of an expert witness may be, and to show also to what extent lawyers may go who look only to the winning of their causes, permit me to refer to an already reported poison case in which I was employed by the people. It may be roughly outlined as follows:

Much arsenic and a very little zinc were found in the stomach.

The body had not been embalmed, but cloths wrung out in an embalming fluid containing zinc and arsenic had been spread upon the face and chest.

Medical testimony showed that no fluid could have run down the throat. Knowing the relative proportions of zinc and arsenic in the embalming fluid, the quantity of arsenic found in the stomach was twelve times larger than it should have been to have balanced the zinc also there present, assuming them to have both come from the introduction of the said embalming fluid by cadaveric imbibition. Other circumstantial evidence was greatly against the prisoner.

At the time of my appearing for the people, on the occasion of the first trial of the case, my direct testimony brought out very strongly the fact that a fatal quantity of arsenic had been found

in the stomach, but no opportunity was given me to testify to the presence of the zinc found there as well, although the fact of its existence in the body was known to the prosecution through my preliminary report. Through ignorance of the nature of such report on the part of the defence, no change was made in the character of my testimony during the cross-examination, and I was permitted to leave the witness-stand with a portion of my story untold. No witnesses were called for the defence, and the case was given to the jury with the darkest of prospects for the prisoner.

For many reasons, unnecessary to recount here, I was distinctly of the opinion that murder had been committed, but I felt nevertheless that common justice demanded that the prisoner should have been entitled to whatever doubt could have been thrown upon the minds of the jury, no matter how far-fetched the foundations for such doubt might have been.

The first trial having resulted in a disagreement of the jury, I was pleased to learn, before the second hearing of the case began, that the defence was prepared to go into the question of the embalming fluid; for the responsibility of permitting only a part of what I knew to be drawn from me, to the entire exclusion of the remaining portion, was greater than I wished to assume. The nature of my report to the coroner having been established, and certain opinions relating thereto having been fully ventilated the jury were possessed of "reasonable doubt" and acquitted the prisoner. What now were the duties of the expert upon the occasion of the first trial of this case and how should he have construed the meaning of his oath?

One eminent legal light, to whom the question was referred, held that the expert was distinctly the property of the side employing him, and that his duty was simply to answer truthfully the questions put to him, without attempting to enlighten the court upon facts known to him, but not brought out by the examination, no matter how vital such facts might be.

Another held that although the above course would be proper in a civil case, yet, in a matter involving life and death, the witness should insist upon the court becoming acquainted with his whole story. Do not such differences in legal opinion make it very desirable that the expert, at least in capital cases, should be the employee of the bench rather than of the bar, in order that whatever scientific investigations are made may be entirely open to public knowledge and criticism?

Although the expert should earnestly strive to have what he has to say presented in the best form, he must remember that to secure clearness, particularly before a jury, technicalities should be reduced to a minimum. To a degree they are unavoidable, but let them be as few as possible. Illustrations should be homely and apt; capable of easy grasp by the jury's minds, and if possible taken from scenes familiar to the jury in their daily lives.

It is an unfortunate fact that the expert must be prepared to encounter in the court-room not only unfamiliarity with his specialty, but also deep-rooted prejudices and popular notions hoary with age and not to be lightly removed from the mind by the words of a single witness. As President Jordan has well said: "There is no nonsense so unscientific that men called educated will not accept it as science," and, let me add, they will calmly attempt to shove the burden of proof upon the scientific man who is opposed to their views. Sanitary experts, in particular, run up against all sorts of popular superstitions and are inveighed against as "professors" by those who consider themselves the "practical" workers of the time; and, let it be noted, the burden of proof is uniformly laid upon these "professors'" shoulders, while the most astounding and occult statements made by the "practical" men may be received without verification.

One source of trouble, which perhaps is peculiar to the water expert, lies in the impossibility of utilizing analytical results such as were made many years ago.

Those who are not chemists fail to grasp the fact that the examination of water may not be looked upon from the same point of view as the analysis of an iron ore. The statement that water analysis is but of recent birth, and that it is yet in its infancy, is hard for them to appreciate holding, as they naturally do, that what was true twenty years ago must be true to-day, if science does not lie.

A pit into which many an expert witness falls is prepared for him by insidious questions leading him to venture an opinion upon matters outside of his specialty. It is a fatal error to attempt to know too much. Terse, clear answers, well within the narrow path leading to the point in question, are the only safe ones; and when the line of inquiry crosses into regions where the witness feels himself not truly an expert, his proper course is to refuse to testify outside of the boundaries of his legitimate province.

Unfortunately the expert is as often invited to take these collateral flights by the side employing him as by the opposition.

Affidavits in submitted cases are commonly written by the lawyers and not by the expert, although they are, of course, based upon his reports. In the strength of his desire to win the case, the lawyer often prepares a much stronger affidavit than his witness is willing to swear to.

The writer has had no little difficulty just at this point, and has had plenty of occasion to observe the irritation displayed by counsel upon a refusal to endorse statements which have been "too much expanded."

Every expert-witness, especially in his early cases, is sure to have adverse authorities quoted against him; therefore it behooves him to be so familiar with the literature of his subject as to be capable of pointing out that such and such a writer is not up to date, or that such and such a passage, if quoted in full, would not bear the adverse construction that its partial presentation carries. When the expert reaches a position of such prominence that he can state a thing to be so because he says it, irrespective of whatever may be written on the subject to the contrary, his course then is greatly simplified; but long before he attains that altitude he will have put himself upon record in many cases, and happy for him if the record so made be such as cannot be quoted to his disadvantage.

"If I had only not written my first book," is the reflection of many a distinguished author, while one of the great masters of music, referring to an opera, said:—"It is one of my early crimes."

Above all things, the expert "should provide things honest in the sight of all men."

It is well for him to be deeply interested in his case, to feel in a measure as if it were his own, but it is unwise in him to become so partisan as to let his feelings affect his good judgment, and it would be indeed criminal should he permit his interest in any way to contort the facts.

Before the case is brought to a final hearing, it may be apparent that experiments before the court are possible and they may be demanded by the counsel in charge of the case. If such experiments be striking, easy of execution, and not too long, by all means make them.

Practical illustrations, particularly such as involve some fundamental principle, have great weight with the court; but these illustrations must not be such as would turn the court-room into a

temporary laboratory and involve the loss of much time in vexatious waitings.

Such experiments as are determined upon should be thoroughly rehearsed beforehand, no matter how simple they may be; for, of all failures, the court-room experiment which declines to "go off" is perhaps the most dismal.

This brings to mind a kindred topic upon which there should be a word of caution: laboratory experiments, which work to perfection, may utterly fail when expanded to commercial proportions, so that it is wise to bear in mind the danger of swearing too positively as to what will happen in large plants, when the opinion is based only upon what is observed to occur upon the smaller scale. Like conditions will, of course, produce like results, but it is marvellous how insidiously unlooked-for conditions will at times creep into one's calculations, and how hard it is even to recognize their presence.

When preparing his case for presentation, the expert often errs in not dwelling more largely upon certain points because he thinks them already old and well known. To him they may be old, but to the public they may be of the newest. Not only is the public unequally posted with the specialist but what it once knew upon the subject may have been forgotten. It is well, therefore, to insert, in a special report, matters that would be properly omitted from a paper prepared for a professional audience.

Sanitary problems are of especial interest to the public, but the amount of ignorance, or rather false knowledge, displayed concerning them is surprising and often difficult to combat. The sanitarian is not unfrequently called upon suddenly to defend a position involving complex statistics; and, because the data cannot be forthwith produced, the inference is drawn that his points are really without facts to support them and that they are consequently not well taken.

Long before he gets into court, particularly if the time for preparation of the case be short, the expert may well "pray to be delivered from his friends." He may receive a peremptory order by telegraph to "determine the mineral qualities of this rock," when the telegram should have read "Assay this ore for silver," and later it may be a matter of surprise that a quantitative knowledge of the copper present was not obtained while passing along the line for the determination of the silver; for it is generally not

known that the complete analysis of anything is quite rare and correspondingly tedious and expensive.

Toxicologists who hear me may call to mind some case involving a search for the presence of an alkaloid, strychnia for example, during which search, the district attorney, in his eagerness for information, may have asked to know what the indications were as to the presence of the poison, at a time when the extraneous organic matter was not nearly removed. He may have wished no final report, but only the simple probabilities, whereon to base a possible arrest. Such requests are very common, and are akin to a demand for a proof of the pudding during the early baking, when we all know that such proof comes at a much later stage of the proceedings.

Finally, "When doctors disagree who shall decide?"

This question is often very vigorously settled by the jury, as was instanced in a recent celebrated murder trial in New York City. In that case what the experts had to say on either side was simply thrown overboard as a whole, and the finding was based upon the testimony of the remaining witnesses.

What can be said upon this question of the disagreement of expert witnesses? First, it must be noted, they are far from being the only class of people who fail to agree, and that too on very important subjects. Do my hearers think it would be a very difficult task to find a small army of men who would testify very variously and very positively upon questions of politics or religion? Would it be hard to find "good men and true" who would give under oath greatly differing opinions concerning the propriety of instituting free trade or establishing an inheritance tax? Experts are subject to the same errors of judgment as befall the rest of professional humanity, and, when their opinions clash, they are entitled to the same respect that we grant to the members of the bench when they hand down the decision of a divided court.

One fruitful opportunity for disagreement always arises when questions are brought into court touching upon matters newly discovered and apart from the well beaten path of common professional knowledge. Doubt is often left upon the minds of those seeking the light, even when the testimony is given by the specialist who originally developed the new point in question, for one cannot be expected to be thoroughly educated in that which he has himself but recently discovered.

Many of us have dreaded to see the "ptomaines," or putrefactive alkaloids make their way into court with their mystifying influences upon judge and jury and their tendency to protect crime. Now that they are in, what is to be the end? Even with no "Ptomaine theory" possible, the ptomaine form of argument is not unknown. The writer was once asked in an arsenic case, whether he was willing to swear that at some future time an element would not be discovered giving the stated reactions now called arsenical. Such nonsense is of course instituted to impress the jury, and is suggested by similar questioning in the alkaloid cases.

A recent and somewhat amusing instance arose from an attempt to introduce the rather new conception of "degeneracy" into a murder trial. The defence sought to show that the prisoner was a "degenerate" and offered expert testimony as to the meaning of the term and as to the signs whereby such a condition was to be recognized; whereupon the prosecution called attention to the fact that the defendant's experts themselves exhibited every one of the signs in question.

Having said all that he was to say, and having stated it to the best advantage, should the expert depend upon the stenographer's so recording it as to allow of its being used in future without correction? Decidedly not.

The average stenographer is unfamiliar with technical terms, especially such as are chemical, and the witness who fails to supervise the minutes may find out later that he was sworn to a most remarkable array of "facts." The writer once discovered that he had recommended, as a very efficient method of purifying a city water, the filtering of the entire supply "through a layer of black mud." Not to take your time further, let us summarize what has thus been briefly said:

The expert witness should be absolutely truthful, of course; that is assumed, but beyond that he should be clear and terse in his statements, homely and apt in his illustrations, incapable of being led beyond the field in which he is truly an expert, and as fearless of legitimate ignorance as he is fearful of illegitimate knowledge.

Mounting the witness-stand with these principles as his guide, he may be assured of stepping down again at the close of his testimony with credit to himself and to the profession he has chosen.

PAPERS READ.

[ABSTRACTS.]

UPON THE COMPOUNDS OF HYDRONITRIC ACID. By Prof. L. M. DENNIS and Dr. C. H. BENEDICT, Cornell University, Ithaca, N. Y.

[This paper will be printed in Journal of American Chemical Society.]

ACTION OF OXIDE OF MANGANESE ON POTASSIUM PERMANGANATE. By Dr. CHARLES L. REESE, Ph.D. and H. N. MORSE, Johns Hopkins University, Baltimore, Md.

A COMPARISON of the action of the oxide of manganese formed by precipitation from potassium permanganate with manganese sulphate, hydrogen and sugar when agitated in sealed tubes from 24 to 600 hours without and with various quantities of sulphuric acid.

Permanganate was shaken in tubes alone and with sulphuric acid to show that when these substances are free from oxides of manganese no reduction takes place except in strong solutions with acid equal to three times the potassium in the permanganate.

Tubes were also agitated simultaneously containing permanganate acidified with sulphuric acid, with hydrogen, manganese dioxide from manganese sulphate and sugar. The rates of reduction under similar conditions are compared.

[This paper will be printed in the American Chemical Journal.]

CONTRIBUTIONS TO THE CHEMISTRY OF DIDYMIUM. By Prof. L. M. DENNIS and E. M. CHAMOT, Cornell University, Ithaca, N. Y.

[This paper will be printed in Journal of American Chemical Society.]

ON TWO POLYMERIC SERIES OF PHOSPHORUS-NITROGEN COMPOUNDS AND ON THE STEREOCHEMISTRY OF PHOSPHORUS AND NITROGEN. By Dr. H. N. STOKES, Washington, D. C.

IN the series of phosphorus compounds described, which have the general formula $(PNCl_2)_n$ and which are termed *phosphonitrilic chlorides*, the members are easily interconvertible, no other agency being required than heating followed by fractional distillation. The compounds are mostly well characterized and give, on decomposition by water under certain conditions, a polymeric series of acids $(PNO_2H_2)_n$, the *metaphosphimic acids*. The members thus far prepared are:

SPECIMENS.	CHLORONITRIDES.	ACIDS.
Triphosphonitrilic chloride . . .	$(PNCl_2)_3$	$(PNO_2H_2)_3$
Tetraphosphonitrilic chloride . .	$(PNCl_2)_4$	$(PNO_2H_2)_4$
Pentaphosphonitrilic chloride . .	$(PNCl_2)_5$	$(PNO_2H_2)_5$
Hexaphosphonitrilic chloride . .	$(PNCl_2)_6$	$(PNO_2H_2)_6$
Heptaphosphonitrilic chloride . .	$(PNCl_2)_7$	$(PNO_2H_2)_7$
. . . .		
Polyphosphonitrilic chloride . .	$(PNCl_2)_n$	

Besides the above a residuum is obtained which analysis shows to consist of a mixture of members higher than hepta, but which have not been separated owing to practical difficulties, caused by their high boiling points and the consequent changes which they undergo on attempting to separate them by distillation. Any one of these bodies, on heating to a temperature of 350°, is converted into the last, polyphosphonitrilic chloride.

A discussion of probable structural formulæ for the above bodies followed.

THE ACTION OF IRON AND OF ELECTROLYTIC HYDROGEN ON NITRIC ACID. By GEORGE O. HIGLEY, Ann Arbor, Mich.

[*Communicated by the Secretary.*]

THE products of reduction of nitric acid with iron are nitrogen dioxide, nitric oxide and with dilute acids, nitrogen and ammonia. Nitrous oxide is not formed in large quantities as is the case with lead and copper.

Electrolysis of dilute nitric acid mainly produces hydrogen.

[This paper will be printed in the American Chemical Journal.]

THE ACTION OF NITRIC ACID ON ALUMINIUM AND THE FORMATION OF ALUMINIUM NITRATE. By J. B. STILLMANN, Minneapolis, Minn.

ALKYL BISMUTH IODIDES. By Prof. A. B. PRESCOTT, Ann Arbor, Mich.

QUATERNARY alkyl iodides give a bright colored precipitate with bismuth salt solutions. Formed from tetramethyl ammonium iodide, from whatever bismuth salt solution, the composition we find to be $N_3 (CH_3)_{11} H Bi_2 I_7$. The compound crystallizes best from potassium iodide solution acidulated with hydrochloric acid. in hexagonal plates and prisms, of a bright dark-red color. Formed in acidulated solution of bismuth chloride, it is of bright yellow color, and gives a good qualitative analytical indication. A corresponding compound was obtained when pyridine salts were treated with potassium bismuthic iodide (Dragendorff's alkaloidal reagent). This product crystallizes also in hexagonal plates. Composition $(C_5H_5N)_3(HI)_3 Bi_2 I_6$.

Kraut has reported (Annalen, *210*, 319) the corresponding piperidine compound to be $(C_5H_{11}N)_3 (HI)_3 Bi_2 I_6$. The precipitate formed by Dragendorff's reagent and atropine salts we find to have the composition $C_{17}H_{23}NO_3)_3(HI)_3 Bi_2 I_6$. Those of brucine, and of strychnine, respectively $(C_{23}H_{26}N_2O_4)_3 (HI)_3 Bi_2 I_6$, and $(C_{21}H_{22}N_2O_4)_3(HI)_3 Bi_2 I_6$. Therefore, accounting these compounds as "double salts" in the sense of halogen addition compounds, these formulæ are consistent with their obvious constitution, in one order for all pyridine derivatives, and in another order for fatty alkyl compounds.

[This paper will be printed in the Journal of the American Chemical Society.]

KOLATANNIN. By Prof. A. B. PRESCOTT and J. W. T. KNOX, Ann Arbor, Mich.

THE authors find all the tannin of the kola nut, that combined with caffein and that uncombined, to be a single chemical individual, distinct from any tannins previously found in beverage plants or elsewhere. It adheres to the "oak tannin group," not to the "gall tannin group." It is homologous with the caffetannic acid of Hlasiwetz, but on the contrary is not a glucosid. It purifies to perfect constancy of composition, $C_{16}H_{20}O_8$: yields at once a pentacetyl derivative; yields also successively tri- tetra- penta- and with difficulty a hexabromine substituted derivative. These substitution products are the same whether acetyl or bromine be substituted first in order. But the hexabrom kolatannin forms a tetracetyl derivative instead of a pentacetyl. Kolatannin and its bromderivatives form first to fourth anhydrides. By boiling with dilute acids it forms a "red," which is a mixture of two or more bodies of variable composition, not justifying the kola-red of Knebel and Hilger. Kolatannin can be made to yield both protocatechuic acid and phloroglucin. The authors desire to determine other decomposition products before deciding

upon the most probable constitutional formula, which must be of interest, first, in the pure chemistry of the tannins at large, and second, in the physiological chemistry of the caffein compounds of the beverage-plants. This paper is in continuation of that of the authors in 1896: J. Am. Chem. Soc., *19*, 63.

[This paper will be printed in the Journal of the American Chemical Society.]

ON THE CHEMISTRY OF METHYLENE. By Prof. J. U. NEF, University of Chicago, Chicago, Ill.

THE existence of two series of hydrocarbons, C_2H_2, and of their halogen substitution products has been established by experiment, namely: I, of acetylidene, $CH_2=C$; of chlor- brom- and iodoacetylidene, $CHX=C$ (where $X=Cl$, Br or I); of diiodoacetylidene, $I_2C=C$. These substances all contain bivalent carbon; they are all very poisonous compounds, spontaneously combustible and enormously reactive. Most thoroughly studied thus far have been diiodoacetylidene (formerly regarded as diiodoacetylene, $IC\equiv CI$, and which is strikingly like phenylisocyanide, $C_6H_5N=C$, in odor and in its reactions; and monobromacetylidene which has previously been supposed to be bromacetylene, $BrC\equiv CH$; this substance resembles poisonous phosphorus in a remarkable manner and might consequently well be termed vegetable phosphorus.

It has also been made highly probable that the acetylene salts are derivatives of acetylidene, *e. g.*, that calcium carbide has the formula $Ca=C=C$; the salt is consequently entirely analogous to cyanide of potash $KN=C$, in structure. It is the intention of the author to develop the chemistry of acetylidene and of its substitution products to the fullest extent.

II, of acetylene $CH\equiv CH$, and its mono- and di-halogen substitution products, $X-C\equiv CH$ and $X-C\equiv CX$ (where $X=Cl$ Br or I). These are all sweet smelling, harmless compounds, which possess very slight chemical activity compared with the isomeric acetylidene derivatives. At the present time the only halogen substitution product of acetylene known is iodoacetylene, $CI\equiv CH$. This is a sweet smelling liquid (b. pt 29–32°) discovered recently by Paterno: it is indifferent towards an excess of iodine. The isomeric iodacetylidene $CHI=C$, on the other hand, is a frightfully poisonous, extremely reactive substance, which will be exhaustively studied during the coming year.

It is the purpose of the author to isolate in the future, if possible, all the isomeric compounds demanded from the theoretical standpoint.

[This paper will be printed in Liebig's Annalen der Chemie; also by the University of Chicago.]

ON THE CONSTITUTION OF SOME HYDRAZONES. By Prof. PAUL C. FREER, Ann Arbor. Mich.

THE hydrazone of acetone is brominated to *p* brom acetone hydrozone; it is then oxidized to an explosive azo body by air; the same result is true of other hydrazones, excepting that of pyruvic acid.

[This paper will be printed in the American Chemical Journal.]

DETERMINATION OF THE VOLATILITY OF PHOSPHORUS PENTOXIDE. By Prof. E. W. MORLEY, Cleveland, Ohio.

ON THE ACTION OF SODIUM ON METHYLPROPYLKETONE AND ON ACETOPHENONE. By Prof. PAUL C. FREER, Ann Arbor, Mich.

METHYLPROPYLKETONE and acetophenone yield sodium derivatives which react readily with benzoyl chloride to form dibenzoylmethylpropylketone and the benzoate of isomethylpropylketone and from acetophenone, tribenzoylmethane and the benzoate of tribenzoylmethane. In addition there are considerable quantities of halogen containing oils formed.

[This paper will be printed in the American Chemical Journal.]

THE DECOMPOSITION OF HEPTANE AND OCTANE AT HIGH TEMPERATURES. By A. W. BURWELL, Ph.D., Ithaca, N. Y.

DERIVATIVES OF ENGENOL. By F. J. POND and F. F. BEERS.

(Read in Joint-Session with American Chemical Society.)

RECENT PROGRESS IN ANALYTICAL CHEMISTRY. By Prof. L. M. DENNIS, Cornell University, Ithaca, N. Y.

A review of the progress in analytical chemistry for the past eighteen months.

QUALITATIVE ANALYSIS, A POINT IN TEACHING THAT WAS NOT A FULL SUCCESS. By Prof. A. L. GREEN, Purdue University, Lafayette, Ind.

A NEW COLOR STANDARD. By ELLEN H. RICHARDS, Massachusetts Institute of Technology, Boston, Mass.

NOTES ON THE EVIDENCES OF DISSOCIATION IN DILUTE SOAP SOLUTIONS. By ELLEN H. RICHARDS, Massachusetts Institute of Technology, Boston, Mass.

A COMPARISON OF METHODS OF DETERMINING CARBON DIOXIDE AND CARBON MONOXIDE. By Prof. L. M. DENNIS and C. G. EDGAR, Cornell University, Ithaca, New York.

[This paper will be printed in the Journal of the American Chemical Society.]

A PRELIMINARY THERMO-CHEMICAL STUDY OF IRON AND STEEL. By Prof. E. D. CAMPBELL and FIRMAN THOMPSON, Ann Arbor, Mich.

[This paper will be printed in the Journal of the American Chemical Society.]

FURTHER STUDY ON THE INFLUENCE OF HEAT TREATMENT AND CARBON UPON THE SOLUBILITY OF PHOSPHORUS IN STEEL. By Prof. E. D. CAMPBELL and S. C. BABCOCK, Ann Arbor, Mich.

[This paper will be printed in the Journal of the American Chemical Society.]

ON THE CHEMICAL COMPOSITION OF CEMENT PLASTER. By Prof. E. H. S. BAILEY, State University, Lawrence, Kansas.

THE material used for making cement plaster is found over large areas in Kansas, Indian Territory and Texas. It is an earthy powder, composed of sand, clay, calcium carbonate, and a varying quantity of calcium

sulphate. The composition in a single bed varies greatly in different parts. The process of manufacture of the plaster is described, and a series of analysis is given of the crude material, the finished plaster, dust, tailings and the "set" cement. Some of the calcium is no doubt present in combination with silica. The amount of water in the set material is practically the same as in the original gypsite or crude material.

[This paper will be printed in the Report of the Geological Survey of Kansas.]

On reactions between mercury and concentrated sulphuric acid. By Prof. Charles Baskerville and F. W. Miller, University of North Carolina, Chapel Hill, N. C.

If mercury in excess be heated with concentrated sulphuric acid, mercurous sulphate, sulphur dioxide and water are produced, it matters not at what temperature the reaction is caused to take place. If the acid, however, be in large excess at 100° C. mercurous sulphate is produced with some mercuric sulphate. At 150° C. the crystalline compound produced possesses the formula, $Hg_3 (SO_4)_2$ or $Hg_2 SO_4 . HgSO_4$. If the temperature be increased the amount of mercurous sulphate decreases until the reaction is carried out at the boiling point of the acid when mercuric sulphate alone is produced. No hydrogen, hydrogen sulphide or free sulphur is formed.

[This paper will be printed in the Journal of the American Chemical Society.]

On the production of a carbohydrate from egg albumin. By Dr. John G. Spenzer, Cleveland, O.

The statement has been made by Pavy, that egg albumin will, when boiled first with potassium hydroxid solution and then with dilute sulfuric acid, furnish a carbohydrate similar to or identical with the *animal gum* of Landwehr. Schützenberger also said albumin heated with dilute sulfuric acid or barium hydroxid solution gives a strongly reducing substance resembling glucose. Egg albumin freed from cell membrane, globulin sugar and ovamucoid, constituents proven by Mörner to exist in hen's egg, gave no such body by the treatment designated. Ordinary impure albumin, however, does present the characters already mentioned by Pavy and Schützenberger.

The writer is of the opinion, based on his observations, that the carbohydrate either preëxists in the white of egg, or is produced from the ovamucoid by the action of hot dilute mineral acids.

ON THE ACTION OF WATER ON HALOGEN SUBSTITUTED ALIPHATIC ACIDS. By E. A. DEBARR, Ann Arbor, Mich.

(Read in Joint-Session with American Chemical Society.)

THE decomposition of mono- di- and trichloracetic acids and of the corresponding brominated products were described, with curves showing the velocity and limits of the reactions in question.

[This paper will be printed in the American Chemical Journal.]

ON THE DETERMINATION OF FAT IN FÆCES. By HERMAN POOLE, New York City.

[*Communicated by the Secretary.*]

[This paper will be published in the Journal of the American Chemical Society.]

APPARATUS FOR PHOTOMETRIC DETERMINATION OF LIME AND SULPHURIC ACID. By Prof. J. I. D. HINDS, Cumberland University, Lebanon, Tenn.

(Read in Joint-Session with American Chemical Society.)

THE APPARATUS.

THE apparatus consists of a cylinder twenty centimeters high and four centimeters wide, graduated from the inner surface of the bottom in centimeters and tenths. In connection with this a common candle and a lipped beaker are used

For Sulphuric Acid. Add to a sufficient quantity of the water to be tested a few drops hydrochloric acid and then enough solid barium chlorid to precipitate all the sulphuric acid. Mix well by pouring back and forth between cylinder and beaker. Holding the cylinder some twelve inches above the lighted candle and looking downward pour the liquid into the cylinder until the image of the candle flame just disappears; then read the height of the liquid in the cylinder.

For Lime. Precipitate the lime with solid ammonium oxalate and proceed in exactly the same manner as indicated above.

[For full discussion of the method and results see author's paper in Journal of the American Chemical Society, vol. 18, no. 8, August, 1896.]

CONSTITUTION OF ARSENOPYRITE. By Prof. EDGAR F. SMITH, University of Pennsylvania, Philadelphia, Pa.

IN a verbal communication the author gave evidence of the presence of a large quantity of ferrous iron in arsenopyrite, showed that the arsenic was present in its trivalent form, and that the sulphur content of the mineral was quantitatively expelled when the latter was heated in hydrogen.

ON SOLUTIONS OF SILICATES OF THE ALKALIES. By LOUIS KAHLENBERG, Ph.D., and AZARIAH T. LINCOLN, B.S., University of Wisconsin, Madison, Wis.

SOLUTIONS of pure silicic acid were prepared by treating sodium silicate with excess of hydrochloric acid and dialyzing — method of Graham. Solutions of pure alkaline hydroxides were also prepared. These solutions were then mixed in equivalent proportions to form clear solutions of known strength of silicates of the formulæ $M\,H\,Si\,O_3$, M_2, $Si\,O_3$, $M_2\,Si_5\,O_{11}$, where M is either Na, K, Li, Rb, or Cs. It was established experimentally that a solution of an alkaline silicate formed by dissolving the salt obtained by fusing together alkali and silicic acid is identical with that obtained by adding to a colloidal solution of silicic acid the proper amount of caustic alkali. The lowering of the freezing point of the solutions was determined by Beckmann's method at various dilutions. The strongest solution tested contained one-eighth of the mol. wt. of the salt in grams per liter. The results obtained show that these silicates are very largely hydrolytically dissociated in solutions into caustic alkali and colloidal silicic acid. This conclusion is of importance to the geologist as well as to the chemist as it increases our knowledge of the condition in which silicic acid exists in natural waters. The electrical conductivity of these silicate solutions was also determined. The results obtained confirm those of the cryoscopic work. Further investigations on other physico-chemical properties of the solutions are still in progress.

In presenting the results charts were used.

[This paper will be printed in Journal of Physical Chemistry.]

A NEW FORM OF DISCHARGER FOR SPARK SPECTRA OF SOLUTIONS. By Prof. L. M. DENNIS, Cornell University, Ithaca, N. Y.

[This paper will be published in the Journal of American Chemical Society.]

THE ELECTRICAL CONDUCTIVITY AND THE ELECTROLYSIS OF CERTAIN SUBSTANCES DISSOLVED IN LIQUID AMMONIA. By HAMILTON P. CADY and E. H. S. BAILEY, State University, Lawrence, Kansas.

THE remarkable similarity existing between certain crystalline salts containing water of crystallization and similar salts containing ammonia, has suggested the idea that perhaps water and ammonia might be analogous in some of their other properties. Acting on this suggestion it was thought best to test the dissociative power of liquid ammonia on dissolved substances.

The experiments were made in vacuum-jacketed tubes, and the liquid ammonia used was the ordinary commerical material used by ice manufacturers. It was found that the liquid ammonia did not conduct the electric current; but, if salts such as ammonium chloride be dissolved in the ammonia, the current is conducted. Salts of various metals were dissolved in the liquid ammonia, and the products of the electrolysis, which takes place when the current is passed through the solution, were studied.

Ammonia will dissolve metallic sodium, and then the solution becomes an excellent conductor, but no gas is evolved. and there is no deposit on the electrodes. There are no signs of a polarization current, and it was noticed that the molecular conductivity rises with the concentration. The solution acts then like a metal and not like an electrolyte.

From experiments on the solution of various salts in liquid ammonia it is seen that the latter has the power of dissociation of dissolved substances to as great extent as water, and the resemblance between ammonia and water as solvents is much greater than supposed.

THE RATE OF SOLUTION OF SOLID SUBSTANCES IN THEIR OWN SOLUTIONS. By A. A. NOYES, Ph.D. and W. R. WHITNEY, Ph.D., Massachusetts Institute of Technology, Boston, Mass.

CAST sticks of benzoic acid and of lead chloride were rotated in bottles of water in a thermostat for varying lengths of time. The resulting solutions were analyzed and from the mean values of many determinations the law was established that:

The velocity of solution of a solid substance in its own solution is proportional to the difference between the concentration of this solution and that of the saturated solution.

[This paper will be printed in the Journal of the American Chemical Society.]

On a method for observing the rate of action of a liquid upon a solid. By A. Vernon Harcourt, Reader in Chemistry at Christ Church, Oxford, England.

The author drew and described the apparatus which he had devised for making observations of the rate of action, under constant conditions, of various liquids upon various substances which could be brought into a spherical form. The liquid was driven rapidly backwards and forwards in a U-tube in one limb of which the small solid ball was supported on three projections. A glass float struck the ball from beneath each time that the liquid oscillated, and changed its position. The volume of liquid was very large relatively to the mass of solid dissolved in each experiment, so that the liquid was practically of constant composition. The temperature was kept constant by the immersion of the U-tube in a large vessel of water the temperature of which was carefully set and regulated.

At the beginning and end of each experiment the small sphere was weighed, and from these weighings the density of the substance having been determined, the mean area exposed to the action of the liquid was calculated

The motor employed to drive the liquid to and fro was a form of hot-air-engine which the author had devised It can be made and put together in a quarter of an hour, consisting of a piece of combustion tubing closed at one end and drawn out at the other to half its former diameter at a distance of three inches from the closed end. The drawn out part is bent at right angles and its open end is fixed with a ring of India rubber in one limb of a U-tube of from 100 to 200 c. c. capacity. The U-tube is half filled with mercury and the combustion tube heated to as high a temperature as it will bear without softening. A small oscillation is given to the mercury which then continues to oscillate strongly, alternately driving air into the heated chamber and being driven back by the expansion of the heated air.

Some contributions to methods of testing wheat flour; with illustrations. By Prof. R. C. Kedzie, Agricultural College, Mich.

Recent progress in agricultural chemistry prepared by request of the Secretary of Section C. Prof. H. W. Wiley, U. S. Department of Agriculture, Washington, D. C.

This paper contains a résumé of recent progress in the lines of agricultural chemistry. The method of preparing nitragin and of inoculating seeds and soils therewith is described. A statement is given of the results of Snyder's work in the nourishment of plants with humates. The

effect of vegetable soils in increasing the nitrogenous content of oats is described. The work of Mazé proving that symbiosis is not a necessary condition for the activity of the nitrifying organisms inhabiting the nodules of leguminosæ is noticed. The sources of the original nitrogen first available for vegetable food are pointed out. These sources are doubtless metallic nitrids or some similar combinations. Recent progress in the study of denitrifying organisms is described and the controversy between the French and German schools of agriculture noted. The latest information on the methods of activity of the chlorophyll cells is referred to as illustrating our present knowledge of vegetable metabolism. The work of Wheeler on the acidity of soils is noted and also the method of Tacke of determining the quantity of acid in soils. Reference is made to the discussion which is progressing in regard to the possibility of replacing potash with soda and the most recent facts relating to the subject set forth. A review of the data on this subject shows that there is no valid ground for asserting that soda can in any way essentially replace potash as an element of plant food. The work of Osborne on the separation and study of the properties of the cereal proteids is collated and the factors appropriate for the conversion of nitrogen in the different cereals into protein given. The chief changes from the old factor of 6.25 are found in wheat, rye and barley. The factor for maize, as determined by Osborne, is practically the same as the one which is commonly employed. The importance of the use of basic slags as fertilizing materials is pointed out and the methods of valuation briefly discussed.

CALORIES OF COMBUSTION OF CEREALS AND CEREAL PRODUCTS CALCULATED FROM ANALYTICAL DATA. By Prof. H. W. WILEY and W. D. BIGELOW, U. S. Department of Agriculture, Washington, D. C.

FACTORS for calculating the calories of combustion are given and the results compared with the data obtained by direct combustion. The results of the investigations show that the approximate factors are:

For oil,	9300
" protein,	5900
" starch,	4200

When the calculated calories differ by more than 100 from the numbers directly determined it is advisable to repeat both the analysis and the combustion.

METHODS OF STARCH DETERMINATION IN CEREALS AND CEREAL PRODUCTS. By Prof. H. W. WILEY and W. H. KRUG, Washington, D C.

THE standard methods of starch determination in cereals are enumerated. Comparative determinations of the starch by the different methods

are given. Variations of the methods are described and illustrated. It is shown that all the methods depending on heating in an autoclave at a high temperature, in the presence of an organic acid, give results which are too high, due to the conversion of carbohydrates not starch into reducing sugars. The superiority of the methods depending on digestion with diastase at fifty or sixty degrees is pointed out. A comparison of the activity of ordinary diastase and taka diastase is given. It is demonstrated that the diastase method properly manipulated gives results for starch which are approximately correct, being possibly a trifle too high. It is demonstrated that with careful manipulation by the analytical methods pointed out the sum of the analytical data in the case of a cereal approximates 100 per cent, but that the variation is naturally greater than in the case of a mineral substance.

THE SOLUBILITY OF PENTOSANS IN THE REAGENTS EMPLOYED IN THE ESTIMATION OF STARCH. By W. H. KRUG and Prof. H. W. WILEY, Washington, D. C.

WHEN cereals are heated in an autoclave, in an atmosphere of steam, at a pressure of from two and a half to three atmospheres, for the purpose of rendering the starch soluble, caramelization is likely to occur, unless a minute quantity of some organic acid be present. Lactic and tartaric acids are the ones usually employed, but salicylic acid was found by the authors preferable in most cases. All of these acids, however, exert a great solvent effect on the pentosans which are present in the cereal grains. More than three-quarters of the total pentosans present may be brought into solution by the method mentioned. In the determination of starch, it is necessary to take into account the pentosans passing into solution, since on subsequent hydrolysis they are converted into xylose or arabinose and thus increase the apparent percentage of starch. The assertion of König to the effect that pentosans are also soluble in diastase was investigated. Careful determinations showed only a trace of solubility of the pentosans in this reagent and the analytical data obtained do not agree with those cited by König.

A CONTINUOUSLY REVISED COMPENDIUM OF CHEMISTRY. By E. E. EWELL, Washington, D. C.

THE POISONS OF TUBERCULOSIS BACILLUS. By E. A. DE SCHWEINITZ, Washington, D. C.

(Read in Joint-Session with American Chemical Society.)

THE DILATOMETRIC MEASUREMENT OF THE VELOCITY OF A REACTION. By W. R. WHITNEY, Ph.D., Massachusetts Institute of Technology, Boston, Mass.

THE velocity of the hydrolysis of a sugar solution was dilatometrically observed, using for apparatus a quarter-litre Woulfe bottle, three feet of straight glass tubing of about 2 mm. internal diameter and a thermometer. The bottle was filled with the mixed sugar and acid solution and the contraction in the volume of this mixture noted by the fall of the meniscus in the tube which penetrated a cork in the filled bottle. The constants from the heights of this meniscus and the corresponding times by the formula $C = \frac{1}{t} \lg \frac{s}{(s-x)}$ (where t is the expired time, s the total contraction, and x the contraction at time t) were found to be satisfactory.

[This paper will be printed in the Journal of the American Chemical Society.]

SOME RECORDS OF PROGRESS IN INDUSTRIAL CHEMISTRY. By W. McMURTRIE, Ph.D., New York, N. Y.

A STATEMENT of what has been done in some lines of industrial chemistry during the past eighteen months.

Joint sessions were held with THE AMERICAN CHEMICAL SOCIETY.

FIFTEENTH ANNUAL REPORT OF THE COMMITTEE ON INDEXING CHEMICAL LITERATURE.

The Committee on Indexing Chemical Literature presents to the Chemical Section its fifteenth annual report, covering the twelve months ending August, 1897.

WORKS PUBLISHED.

Recalculation of the Atomic Weights. By Frank Wigglesworth Clarke. New edition revised and enlarged. Constants of Nature, Part V. Smithsonian Miscellaneous Collections, 1075. City of Washington, 1897. pp. vi—370. 8vo.

Index to the Literature of the Periodic Law. In: "Development of the Periodic Law." By F. P. Venable. Easton, Pa., 1896. 12mo.

Partial Bibliography of Argon. By C. Le Roy Parker. Accompanying his paper: "Our Present Knowledge of Argon." J. Am. Chem. Soc. xix, 124 (Feb. 1897).

Bibliography of Agricultural Chemistry (American). Bulletins of the Office of Experiment Stations, United States Department of Agriculture.

In our fourteenth annual report the following correction should be made: *for* Bulletin No. 9 *read* Bulletin No. 19, *and add* Bulletin No. 27 (1896).

A card-index to Experiment-Station Literature is issued by the Office of Experiment Stations; this is sent *gratis* to all the Agricultural Colleges and Experiment Stations in the United States, and is sold to a limited number of individuals and institutions. Eleven thousand cards had been distributed prior to September 1896.

The detailed index included in each volume of the "Experiment Station Record" contains numerous references to chemical articles published by the Experiment Stations in the United States and in foreign countries.

Abstracts of Chemical Work in Agricultural Science, published in: "Experiment Station Record," issued by the office of Experiment Stations, United States Department of Agriculture.

These Abstracts were begun in Vol. I, No 1. (September. 1889). Abstracts of foreign investigations were added beginning with Vol. II, No. 8 (March, 1891) and these have been included, with a quite rapid growth in the field covered. up to the present time. The work is in charge of Dr. E. W. Allen, who is assisted by Mr. W. H. Beal in the departments of fertilizers and soils, and by the Committee on Abstracting of the Association of Official Agricultural Chemists.

The Review of American Chemical Research edited by Prof. Arthur A. Noyes, begun in April, 1895, and formerly published in the "Technology Quarterly," is continued in the "Journal of the American Chemical Society."

Periodicals Relating to Chemistry and Physics in the New York Public Library and Columbia University Library. Bulletin of the New York Public Library, Astor, Lenox and Tilden Foundations. Vol. I. No. 6. June 1897. Page 152.

A very convenient check-list compiled with bibliographical accuracy, especially useful to students residing in New York and vicinity.

Bibliography of the Analysis of Chrome-iron Ore, Ferro-Chromium and Chrome-steel. By S. Rideal and S. Rosenblum. *Chem. News*, Vol. 73, p. 2. (Jan. 3, 1896.)

A Bibliography of the Chemistry of Chlorophyll by L. Marchlewski, accompanies his monograph: "Die Chemie des Chlorophylls." Hamburg and Leipzig, 1895. 8vo.

REPORTS OF PROGRESS.

A Bibliography of the Metals of the Platinum Group, 1748–1896, by Professor James Lewis Howe has been completed, and after examination by your Committee has been recommended to the Smithsonian Institution for publication. The work is now going through the press.

A Review and Bibliography of Metallic Carbides, by Mr. J. A. Mathews, of Columbia University, was submitted to your Committee, and after examination by each member, the MS. was returned to the author for minor improvements. The suggestions of the Committee were promptly accepted by Mr. Mathews, and the revised work has been recommended to the Smithsonian Institution for publication.

A Bibliography of Basic Slags, Technical, Analytical and Agricultural, has been completed by Karl T. McElroy of the Division of Chemistry, U. S. Department of Agriculture. The channel of publication has not been determined.

The second edition of the *Catalogue of Scientific and Technical Periodicals*. 1665–1895, by Dr. H. Carrington Bolton, is entirely printed, but publication is deferred owing to the preparation of a new Library Check List, with which it will be accompanied. The new edition contains 8603 titles.

A Supplement to the Select Bibliography of Chemistry, 1492–1896, has been completed by Dr. H. Carrington Bolton, who has presented the MS. to the Smithsonian Institution. This Supplement contains about 9000 titles, including many chemical dissertations, and is brought down to the end of the year 1896.

Dr. C. H. Joüet reports his *Index to the Literature of Thorium* "nearly finished."

Dr. F. W. Traphagen reports "fair progress" on his *Index to the Literature of Tantalum*.

Mr. George Wagner reports that he has made progress on the *Bibliography of Oxygen*.

Mr. H. E. Brown, under the direction of Professor A. B. Prescott, is preparing a *Bibliography of the Constitution of Morphine and related Alkaloids*.

Professor William Ripley Nichols, of the Massachusetts Institute of Technology, at the time of his death left an unfinished "*Index to the Literature of Carbonic Oxid*;" the MS. was taken in hand by Professor Augustus H. Gill of the same institution, who has done considerable work upon it; he now reports that he is not in a position to finish the task and he is perfectly willing to relinquish the large amount of material accumulated to anyone who would undertake to complete it.

Professor Clement W. Andrews, formerly of the Massachusetts Institute of Technology and now Librarian of the John Crerar Library, Chicago, reports that he is obliged to abandon work on his *Index to the Literature of Milk*, and will be very glad to turn over the material to anyone who cares to undertake to complete the bibliography.

It has always been the aim of the Committee on Indexing Chemical Literature to prevent duplication of work, but failure to inform the Committee of work in progress may defeat this undertaking. An announcement in the Fourteenth Annual Report, of certain work having been nearly completed, surprised a chemist in another part of the country, and has led to the abandonment by the latter of much laborious indexing.

In conclusion the Committee repeats the statement that it labors to encourage individual enterprise in chemical bibliography, and to record in the annual reports works issued and works in progress.

Address correspondence to the Chairman, at Cosmos Club, Washington, D. C.

Committee,
H. CARRINGTON BOLTON, *Chairman*,
F. W. CLARKE,
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A. B. PRESCOTT,
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AND

ENGINEERING.

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ADDRESS

BY

JOHN GALBRAITH

VICE PRESIDENT, AND CHAIRMAN OF SECTION D

THE GROUNDWORK OF DYNAMICS.

THE subject of dynamics is too often treated as if its chief value consisted in the opportunities it affords for familiarizing the student with the operations of the differential and integral calculus. It is regarded as a department of applied mathematics rather than of mechanical science. That this should be the case is unfortunate; at the same time it is not in the nature of things altogether avoidable. The student cannot afford the time involved in deferring the study of dynamics until he has acquired a working knowledge of the calculus. As a consequence he becomes confused respecting the origin of his difficulties, and possibly attributes to his ignorance of mathematics, misconceptions the nature of which may be purely dynamical. It would be of great benefit to him to have the opportunity of attending, before the close of his studies, a short course of lectures on the fundamental principles of the subject, that is to say the conceptions springing directly from experience, upon which the science is founded. The mind of the individual resembles in its mode of growth the mind of the race. The study of the historical development of mechanical ideas will go a great way in clearing up difficulties which arise from adhering too closely to one line of thought. Many of the greatest advances in science are the result of changes of the point of view. Such changes correspond in some measure with the process known to the mathematician as the transformation of coördinates, a process which often transforms a mass of brain-wearying symbols into

ordered groups instinct with life and meaning. It will be well therefore for the student of dynamics occasionally to make the endeavor to transform his coördinates and view the subject from different standpoints. For this purpose it is unnecessary that he should be an expert in refined mathematical analysis. It is requisite, on the other hand, that he should possess in some degree, what may be called the mechanical instinct. The whole power of modern analysis has not proved sufficient to solve, in its generality, the problem of the three bodies ; a problem extremely useful, nevertheless, as an illustration of dynamical principles.

The science of statics, or the laws of composition of forces and couples, was developed from rude experiments made with springs and with strings and weights ; the tensions of the strings being measured by the weights. The conditions necessary in these experiments were, that the body to which the springs or strings were fastened should be maintained by them at rest, and that all changes of shape and size should have ceased. Statics was thus established without the introduction of the ideas of mass and acceleration. In this stage, however, its laws were supposed to apply only to rigid bodies at rest. The facts to be noted are, that force was recognized as a fundamental conception, and that methods of measuring it and the laws of composition of forces were discovered without reference to motion except in the respect that rest was supposed to be a necessary condition.

The connections between force and motion, the subject matter of dynamics, were established by observing the motions of falling bodies, pendulums, bodies rolling and sliding down inclined planes, colliding spheres, bodies connected by strings running over pulleys, etc. In making these experiments it was necessary to know the forces acting. As the only scientific knowledge of force at the time was contained in the laws of statics, the assumption was made that these laws were true even though the bodies were moving and whether the velocities were increasing, decreasing, or changing in direction. This was a change of the point of view which was fruitful in important results. It was found, however, that while the above assumption was justified in the case of the composition of forces, and in the case of weights when considered as forces acting on the heavy bodies themselves, it was not true to assume that the tension of a string in motion is measured by the attached weight. The true indication of the tension in a string was

recognized to be the same as in the case of a spring, viz., the elongation. Again, while in statics the principal objects were the strings and weights, and the bodies to which they were attached were of little or no account, in the dynamical experiments the bodies assumed importance. The conception of mass was introduced, and was found to correspond with the commercial idea of quantity of material, as determined by the balance and weights. The results of these experiments are contained in the laws of dynamics or the laws of motion, as they are usually called, which may be summarized as follows :—

I. That no change in a body's motion of translation takes place except by the action of external force.

II. That external forces impress on a body changes of momentum in their own directions at rates proportional to their magnitudes.

III. That action and reaction are equal and opposite, and in the same line.

It seems to be a matter of doubt, whether Newton, in his statement of the laws of motion, which I have thus paraphrased, intended to affirm that action and reaction are in the same line. Whether there be ground for this doubt or not, the idea is implied in D'Alembert's principle and is accepted as true.

The third law is a law of force, the value of which is seen when the mutual actions of bodies are to be considered. Without this law the laws of statics and of motion would refer to actions on one body only.

It was evident that the laws of composition of forces could be considered as corollaries of the laws of motion; so that from the latter a more comprehensive view of the measurement of force was gained, than from the statical experiments. Thus the statical method of measuring force by balancing the tension of one string or spring against that of another was interpreted dynamically by assuming that the body upon which the springs acted remained at rest in consequence of receiving from the forces opposite changes of momentum at the same rate. It followed that if the springs were to act singly, they would, other things being equal, produce equal rates of change of momentum. Thus dynamics furnished a new method of measuring force which agreed with that by means of springs.

Further investigation in the light of the new principles showed that the method of measuring forces by weights agreed with the

spring measurement and with the dynamical measurement when the condition was observed that the weights were kept in the same locality, but in general not otherwise. Dynamics gave an explanation of this anomaly by showing that the forces of the weights were due to gravity, which evidently might change in intensity from place to place, although the masses of the weights remained unaltered. The measurement of force by weights was thus shown to be a special case of the general dynamical method. Also it resulted that the measurement of force by weights was in principle the same as the measurement by springs, the weight and the earth together constituting a spring whose elastic force is gravity.

It is of little use in the early dynamical training of a student to dwell on the fact that the unit of force in dynamical measure is a force which produces unit acceleration in unit mass. It may even do harm. It may have the effect of leading him to believe that the refined methods of measuring force by means of weights and springs, which are the only methods used in the laboratory, are wrong, and that the testing machines and pressure gauges of the engineer are beneath contempt. It ought to be impressed upon him that all the methods of measuring force, approved by experience, are equally scientific and equally absolute and give exactly the same result, aside from unavoidable experimental error, provided that the proper conditions in each case are observed; also, that the choice of the method depends, just as in the case of everything else in life, upon the objects in view and the circumstances.

We now come to the period of the development and extension of the laws of motion. While proved in the case of small bodies and motions of limited range, it by no means followed that they applied to the motions of the tides or of the planets. Newton by the aid of his happy intuition, the law of gravitation, put new meaning into them and extended their jurisdiction over all visible and measurable motions within the solar system.

All motion is, so we are taught, relative; and the motion which is uniform and in a straight line with reference to one set of axes, may be varying and in a very crooked line when referred to another set, and perfect rest with regard to a third set. The question then is, for what axes are the laws of motion true? It is very certain that the stresses in the springs, by which forces are measured, are in no wise affected by the choice of axes of reference.

Imagine a cube of rubber, with several points marked on its

faces, and consider the lines joining one of these points with the others. The angles between these lines measure the relative directions of the terminal points from the initial point. Now set the cube in motion; stresses and strains are generated, in consequence of which the relative directions of the lines undergo small alterations. Why do these changes of direction remain within small limits? Evidently on account of the nature of the material connecting the points.

Consider now another system of points whose relative directions as time goes on remain almost unchanged, namely, the fixed stars. Is it possible that they may be moving like the points on the rubber cube? Observational astronomy indicates that they are at immense distances from the earth and from each other. If the law of gravitation holds for them their mutual attractions must be so feeble that they form a practically unconnected system. The constancy of their relative directions cannot therefore be accounted for as in the former case by the action of the matter between them, but must be attributable to some other cause. The distinction between the two cases, which is very real, is recognized by assuming that a line may have absolute direction; and the stars are said to preserve their absolute directions from the earth. Again: the assumption that both the law of gravitation and the laws of motion are true for the solar system leads to the result (in consequence of its vast distance from the stars) that its centre of mass has little or no acceleration; in other words, is either at rest or in uniform motion in a straight line.

Now although the ideas of absolute direction and absolute rest or uniform motion in a straight line may, from the kinematical point of view, be incomprehensible, yet in dynamics these terms indicate very definite facts. It is only by the choice of the centre of mass of the solar system as origin and by using the stars to fix the directions of the axes, that it is possible to make the observed motions of the planets fit, at the same time, the law of gravitation and the laws of motion. It is unnecessary to enter into the details of the work; suffice to say, it is a process of trial and error, of assumption, computation and check by observation.

The following dynamical consideration enables, in certain cases, a more convenient origin to be used than the centre of mass of the solar system.

If equal accelerations are impressed on the bodies whose motions

and mutual forces are under consideration, by the attraction of the remainder of the solar system, it is evident that their motions referred to an origin at their common centre of mass, with axes fixed in direction by the stars, will be affected only by their mutual actions. Also, assuming the laws of motion to apply, these motions so measured will show the whole effect of their mutual actions, since the latter have no effect on the motion of their centre of mass. It is thus possible in discussing the lunar tides to use as origin the centre of mass of the earth and moon, and in the case of terrestrial bodies the centre of mass of the earth. It may be noted that whether the primary origin or such subsidiary origins be used, the equations for the mutual forces under consideration are the same; the effect of the change of origin appearing only in the constants of integration. It will be convenient to term all such sets of axes, including the primary set, absolute axes.

It now remains to determine whether the absolute axes will give results agreeing with those already obtained in the small scale experiments, with axes fixed in the earth; and, if they do not, whether the discrepancies can be explained.

If no explanation could be given of such discrepancies, it is evident that the science of dynamics would be resolved into a bundle of empirical rules, describing the various axes of reference that applied in different cases, and the range of applicability in each case.

In order to make the comparison, it will be necessary to obtain the data required for transformation from the absolute set of axes with origin at the earth's centre, to axes fixed in the earth with origin in the locality of the experiments. These data are furnished by astronomical observations. When the transformation is made there appear on the left hand side, let us say, of the equations, the rate of change of momentum of the body relative to the axes fixed in the earth, and on the right hand side the attractions, tensions and other impressed forces, together with certain terms involving the relative motion of the two sets of axes.

In the equations of the original experiments no terms of the latter kind occurred. There are three ways of accounting for the difference. Either the forces are different in the two sets of equations, or the new terms are so small as to be within the limits of experimental error, or each experiment or class of experiments requires its special set of axes.

Experience shows that the explanation lies in the first or second alternative; the third is not true.

These terms are generally negligible in laboratory experiments. It is necessary to consider them in the theory of winds and ocean currents. Their presence in the equations has suggested experiments with pendulums and gyrostats, which confirm their truth. We are justified by experience, for instance, in believing that in the northern hemisphere moving bodies tend to the right, in the southern hemisphere to the left; bodies moving eastward tend to rise, westward to fall; and that bodies, whether at rest or in motion, tend to move outwards from the polar axis. All such tendencies are represented by the terms under consideration. They may be regarded when written on the force side of the equations as representing relative or fictitious forces, fictitious because they correspond to no actions of matter, but are the consequence simply of the motion of the axes of reference relative to the absolute axes.

Sometimes it happens, as has been indicated, that the discrepancy lies in the fact that the forces in the two sets of equations are different, although referring to the same experiment. Consider the case of a body suspended from a spring. Referred to axes fixed in the earth it is at rest and the inference is, that the attraction of the earth is equal and opposite to the tension of the spring. Referring however to the same axes by transformation from the absolute axes, there appears in addition to the terms representing the tension of the spring and the attraction of the earth, a new term, a relative or fictitious force, known as the centrifugal force. The inference now is, that the attraction of the earth is greater than the tension of the spring instead of being equal to it. If this inference be accepted as the true one, the question arises, which of the original forces was wrong, or were both astray? Remembering that the intrinsic indication of the force exerted by the spring is its elongation, and that of attraction the acceleration caused by it, also that acceleration depends on the choice of axes of reference, while the elongation of a spring does not, there can be no hesitation in deciding that the error lay altogether in the estimate of the attraction. The fictitious force, while itself invisible, also rendered invisible a portion of the earth's attraction. By using proper axes of reference its true character is revealed and its power for evil destroyed. If the mechanism of attraction were not concealed, or if it had some distinguishing mark other than accelera-

tion, and it were possible to experiment with it as with springs, such an error could not be made, even with improper axes of reference.

A convenient way of regarding the laws of motion is to consider, as before, the second law as affirming the relation between force and change of momentum and the first and third as asserting the principle of conservation of momentum; the third implying that momentum passes from one body to another without change, and the first that the only way by which the momentum of a body can suffer alteration is by part of it passing into another body. Again, if it be assumed that the third law implies that action and reaction are in the same straight line, the principle of the conservation of angular momentum will follow.

The statement is sometimes made that the "bodies" of Newton's laws must be regarded as particles. I cannot take this view: they are real bodies, of all sizes, and with all the qualities known and unknown of such bodies. They are not the imaginary bodies of the mathematician, the *dramatis personæ* of the algebraic theatre, possessing only the qualities arbitrarily assigned to them for the special purpose of the investigation in hand.

The laws of dynamics thus hold for all bodies within the solar system whose masses, forces and motions have been observed and measured; but the motions must be measured with essential reference to only one set of axes, namely, a set whose origin is in the sun and whose directions are fixed by the stars.

Kinematics deals with relative motion; Dynamics with the *Motus Absolutus* of the Principia.

We now pass to the consideration of the laws of energy in their dynamical relations.

In the discussion of statics as the forerunner of dynamics, attention was directed mainly to the springs, and strings and weights, by which the forces were measured. The original statical experiments may also be regarded as the source of the principles of energy in connection with mechanical science. From this point of view the bodies upon which the forces act come into prominence, not because of their masses as in dynamics, but on account of their shapes, sizes and rigidity. Thus the experiments were made with levers, pulleys, inclined planes, wedges, etc.; in fact, with appliances for doing work, the mechanical powers of the text-books. In the statical principle of virtual velocities we have the origin of

the principle of the equivalence of work and energy. To men of all times the most natural way of regarding force has been, as the action by which material is stretched, bent, twisted, broken or displaced, *i. e.*, whereby work is done. Even the word momentum, in the language of ordinary life, implies the power of doing work. It is worth consideration, whether it may not be better in the instruction of students to work up to the ideas of dynamics through elementary examples of the equivalence of work and kinetic energy, rather than by taking the ordinary balloon passage to the laws of motion. While less systematic and formal this procedure would be more natural and probably more useful.

The laws of energy may be summarized as follows: When work is done on a body an equivalent amount of energy is partly transformed and partly transferred without transformation. It is in general partly transmitted to other bodies with which the given body may be in physical connection. Its transformations are into stored energy and dissipated energy. Examples of stored energy are the potential energies due to gravitation, the forces of elasticity, magnetic and electrical attractions and molecular forces. Such forces are termed conservative. Kinetic energy is another form of stored energy. Energy is dissipated by means of the forces of viscosity and friction, known as dissipative forces. Energy is also stored and dissipated in certain electrical, electro-magnetic, thermal, chemical and other actions which have not been identified with force and which therefore are not dynamical.

In order that work may be done there must be a source of energy, or place from which it comes, and a sink, or place to which it goes, together with an energy stream from the source to the sink. When work is done continuously the energy stream is accompanied by a circuit or system of stored energy which acts automatically as a moderator of its fluctuations.

The principle of conservation affirms that energy can neither be created nor destroyed, so that its changes are changes in form but not in amount.

The principle of the equivalence of work and energy is analogous to the second law of motion, considered as expressing the equivalence of impulse and momentum; that of the conservation of energy has its analogue in the third and first laws of motion regarded as affirming the conservation of momentum.

Newton notices this analogy in his scholium to the laws of

motion in the words, "just as bodies in cases of collision have the same effect, whose velocities are inversely as their masses, so in putting machines in motion, agents have the same effect, whose velocities, in the directions of their forces, are inversely as these forces." The now well-known reference in the same scholium to the action of machines, the importance of which was pointed out in Thomson and Tait's Natural Philosophy, was in continuation of the same line of thought.

The impulse or time integral of a force is fully accounted for by the change of momentum, while the work or displacement integral is only partially accounted for by the change in kinetic energy, in all cases of real bodies. The reason for the difference is that the laws of motion are a complete statement of our experience of force in relation to the motion of a body as a whole, *i. e.*, the motion of its centre of mass. On the other hand, the laws of energy require the consideration not only of this motion but also of all internal motions and forces.

The principle of the equivalence of work and energy is a statement of an effect of force essentially different from its effect in producing change of momentum. It might be supposed, therefore, that this principle would be useful in affording another means of measuring force. The impossibility in general of measuring the whole change of energy due to an unknown force, acting through an observed distance, renders this idea to a great extent, fruitless. If the laws of energy are true, such a method of measuring force must give the same result as the dynamical method. The measurement of force by springs is based on this principle, and not on the second law of motion. Although no attempt is made to measure the change of energy due to the work of extending a spring, yet experience goes to show that the energy changes, due to given extensions made in the same order, are constant and therefore the corresponding forces are constant.

The connection between the laws of energy and those of motion may be stated as follows: Energy and work, like force, are fundamental conceptions gained from experience and having various relations with phenomena which can be discovered only as a result of experiment and observation. One of these relations is that work is proportional to the product of force into displacement. This relation is therefore a natural law, of the same order of importance as the second law of motion, and not a mere verbal definition. Experience thus gives a dynamical measure of work as well as of force.

The law of equivalence of work and energy then establishes work as a dynamical measure of energy.

The laws of motion combined with this law of energy, establish the result that kinetic energy is proportional to the product of momentum and velocity and thus furnish a dynamical method of measuring energy in its kinetic form. This is the sole contribution of the laws of motion to the science of energy.

It is only in the case of bodies whose internal forces and motions are known, or determinable from assumed data, in other words, imaginary bodies, that the laws of energy, as far as they are considered in dynamics, are included in the laws of motion and therefore become unnecessary. except for the purpose of convenience in mathematical analysis or economy of thought. Even in such cases the expressions for work and energy retain a flavor of their original meaning, and do not altogether degenerate into mere mathematical symbols.

The science of dynamics, as it is understood at the present day, includes among its fundamental principles in addition to the laws of motion, the principle of the equivalence of work and energy, and the principle of the conservation of energy; energy being measured, however, only in terms of force and displacement, or momentum and velocity.

The only actions known in dynamics are Force and its integrals Impulse and Work. To identify with these, all other actions involving the transfer and transformation of energy, such as the conduction of heat, chemical reactions, induction of electric currents, etc., forms to-day the severest task of mathematical physics.

PAPERS READ.

[ABSTRACTS.]

A UNIVERSAL ALTERNATOR FOR LABORATORY PURPOSES. By Prof. HENRY S. CARHART, University of Michigan, Ann Arbor, Mich.

THE design of the machine was adopted for the following reasons:

1. Simplicity of construction in the engineering shops without special tools or dies.

2. Its similarity to a bi-polar dynamo so as to illustrate two or three phase current generation, but without the limitation of low frequency.

3. To illustrate the effect of combining E. M. F.'s differing in phase in a variety of ways.

Both the field and the armature are made with poles, the latter having two more than the former. The field is the revolving member and is of the C. E. L. Brown type. The diameter of the armature pole faces is 10.25″; length of poles, 4″; width of poles, 1.5″; depth, 0.5″. The armature poles were wound with 40 turns of No. 16 wire. The double air gap is 0.25″. The field coil contains 1012 turns of No. 16 wire. The armature coils are connected in pairs and the terminals of each pair are brought up to a board on top.

Since opposite poles of the field are of opposite sign, while the corresponding coils of the armature are similarly wound, when the armature is connected as a closed coil, the E. M. F.'s balance exactly as with a bipolar machine. Hence connections may be made for two or three phase as with a bi-polar dynamo.

The coils may also be joined either in star fashion or zigzag across so as to join opposite groups with no phase difference between them.

The phase difference from coil to coil is one-twelfth of a period. Hence the series may be represented by a regular polygon of twelve sides, and the phase difference reduces the E. M. F. of the six coils in series to 0.648 of the E. M. F. without such phase difference.

With 2000 ampere-turns in the field, the following are the observed and computed values of the voltage for the several connections:

Connection.	Observed.	Computed.
Two phase mesh,	79	79
" " star,	112	112
Three " mesh,	69	68.6
" " star,	121	118.8
" " zigzag,	136	136.8

[This paper, with illustrations, is printed in the American Electrician, Nov., 1897.]

SHALL A NEW AND ARBITRARY "ELASTIC LIMIT" BE ADOPTED? By Prof. J. B. JOHNSON, Washington University, St. Louis, Mo.

THE author urges the adoption of an "apparent elastic limit," defined as that stress (or deformation) at which the *rate* of deformation for increasing stresses is 50 per cent. more than it is originally, or for small loads. Arguments in its favor were offered for the purpose of soliciting a discussion.

MACHINE FOR TESTING JOURNAL FRICTION. By Prof. THOMAS GRAY, Rose Polytechnic Institute, Terre Haute, Indiana.

IN this machine the sample journal is mounted on a horizontal shaft and runs in a bearing of any metal desired. The bearing is divided into an upper and lower half carried in a similarly divided box. The two halves of the box are pressed together and the bearing metal caused to press against the journal by means of a pair of heavy levers which grip the box between a pair of knife edges very much as if they formed a large pair of nippers. On these as primary levers, a secondary set of lighter levers are mounted, with their lengths parallel to the axis of the journal, for the purpose of allowing the pressure between the bearing and the journal to be varied and adjusted. The whole system is mounted on knife edges, in line with the axis of the journal, so as to be free to swing in response to the friction. The friction moment is then measured by means of weights as a steelyard lever which is fixed to the lever system with its length at right angles to the axis of the shaft.

The machine is intended to take journals of any diameter up to five inches and of length of bearing up to ten inches. The total pressure may be varied from zero to twenty thousand pounds.

THEORIES OF SOME PLANIMETERS WITHOUT THE AID OF CALCULUS. By Prof. FORREST R. JONES, University of Wisconsin, Madison, Wis.

A GRAPHICAL SOLUTION OF BELTING PROBLEMS. By Prof. JOHN J. FLATHER, Purdue University, Lafayette, Ind.

THE ULTIMATE STRENGTH OF A CONCRETE BEAM WHEN REINFORCED BY A STEEL BAR ON THE TENSION SIDE. By Prof. J. B JOHNSON, Washington University, St. Louis, Mo.

THE strength of a combination steel concrete beam at the time the concrete cracks on the tension side has been solved.[1] But when this condition is reached only about one-third of the ultimate strength of the beam has been developed. It remains to show how to determine the strength of the beam after the concrete has failed on the tension side and for any assumed tensile strength in the steel rod, as at the elastic limit for instance.

To solve this latter case, experiment shows that the following assumptions may be made:

1. The hair-like cracks in the concrete are so frequent that the steel bar may be assumed to be uniformly stretched near the middle of the space and at the same time rigidly attached to the adjacent concrete.

2. Up to the elastic limit of the steel bar, the deformation of the concrete on the compression side, and the stretch of the steel bar on the tension side may be assumed to vary directly as the distance from the (unknown) neutral axis. That is, a section plane before bending remains a plane after bending.

To these must be added a third condition imposed by the laws of mechanics, viz.,

3. The total tensile stress on any cross-section of the beam equals the total compression stress on the same section.

From these we may derive the following:

Distance of Neutral Plane from the Compression Side of Beam =

$$y_1 = \sqrt{\frac{2ae}{b}\,\frac{E_s}{Ec}} \qquad (1)$$

$$\text{Moment of Resistance of Beam} = M = f_s\left(\frac{b y_1^2}{3e}\,\frac{E_c}{E_s} + a e\right) \qquad (2)$$

$$\text{Compressive stress in the Concrete} = f_c = f_s\,\frac{E_c\,y_1}{E_s\,e} \qquad (3)$$

where

a = area of steel bar in sq. in.

y_1 = distance of neutral plane of the cracked beam from the compression side.

e = distance of center of steel bar from the neutral plane of the cracked beam.

b = breadth of beam in question = distance between the steel bars.

E_s = modulus of elasticity of steel.

E_c = " " " " concrete.

f_s = tensile stress per sq. in. in the steel bar.

f_c = maximum compressive stress per sq. in. in the concrete.

[1] In Germany by the method of least work by Julius Mandl in *Baumaterialen Kunde* 1st year, heft 11, *et seq.*; and in this country by the author in his *Materials of Construction* by another method.

The above formulæ are well borne out in practice when it is a case of simple cross-bending, free from all arching action. In deriving these formulæ the tensile stress in the concrete, below the neutral plane and above the bottom of the cracks, is neglected, since it is both small in amount and acts with a very short arm, hence is quite insignificant in making up the moment of resistance. The modulus of elasticity of concrete to use must be taken from a stress diagram obtained from a test of the particular concrete mixtures used, and it must be figured from the total deformation between the origin and the maximum stress developed on the compression side of the beam.

[This paper is printed in Engineering News, Oct. 21, 1897.]

ON ENGINEERING CONDITIONS CONNECTED WITH THE MOUNTING OF INSTRUMENTS USED ON ECLIPSE EXPEDITIONS. By Prof. DAVID P. TODD, Amherst College, Amherst, Mass.

PROMINENTLY they are three:

(1) The first is rigidity — the instruments must not be shaken by gusts of wind, always likely to spring up at the time of the total phase of an eclipse of the sun.

(2) The second is lightness of construction. Very heavy weights are neither economically nor conveniently transported nor installed.

(3) A smooth and perfect clock-work.

The ordinary form of equatorial mounting meets none of these conditions satisfactorily.

One of the objects accomplished by the Amherst Eclipse Expedition to Japan last year, was the devising and construction of a new form of mounting and driving clock which met all these requirements perfectly. Photograph exhibited embodies the final designs. (See Plate.)

Main construction of piping insures lightness and ease of transportation.

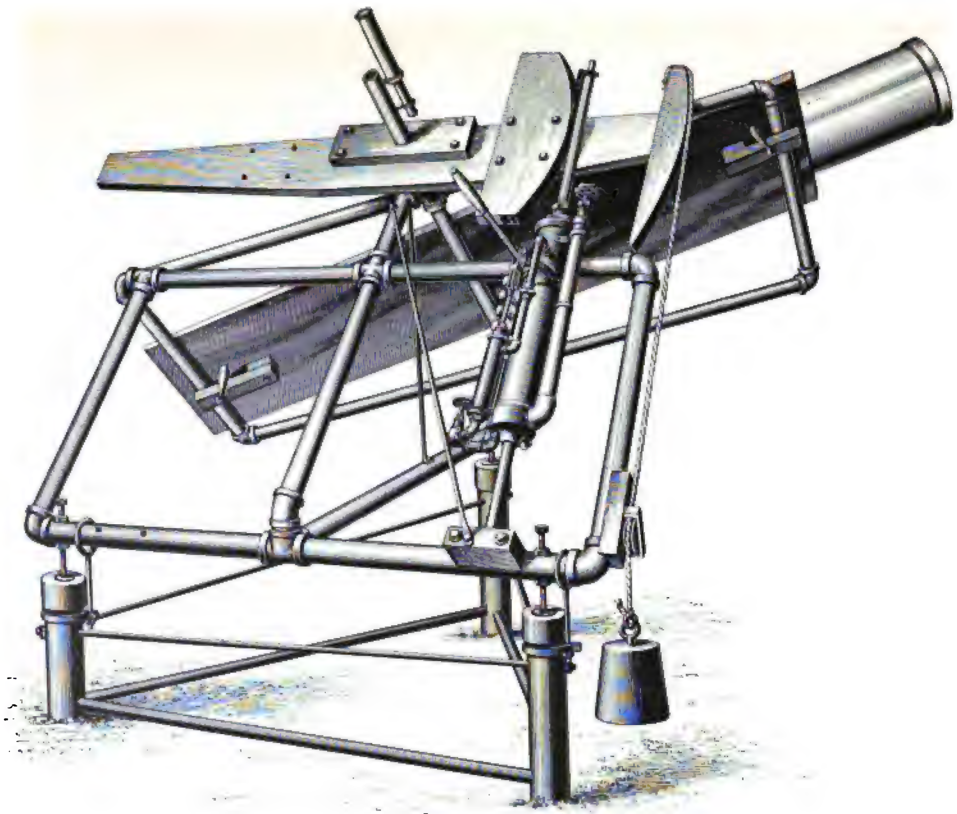
Tripod form of construction, with weight of instruments not counterpoised, but thrown upon the clock, gives a degree of rigidity hitherto unattained, and which ordinary gusts of wind do not in the least disturb.

The novel form of clock motion (an accurately surfaced and smoothly polished cylinder filled with glycerine) affords all desired precision of movement in following sun and stars.

The range of equatorial motion of this form of mounting allows every facility for the needed preliminary test and adjustments.

THE CEMENT LABORATORY AS A FIELD FOR INVESTIGATION. By Prof. FRED. P. SPALDING, Cornell University, Ithaca, N. Y.

NOTWITHSTANDING the large amount of experimental investigation of recent years, knowledge of the nature and properties of hydraulic cement



PORTABLE EQUATORIAL MOUNTING. AMHERST ECLIPSE EXPEDITION TO JAPAN. 1896.

is still in a very crude state. Much difference of opinion exists amongst investigators concerning the reasons for observed phenomena, while engineers differ radically as to the best method of use. The subject presents, therefore, an attractive field for research.

Laboratories, intended for purposes of instruction, exist in many engineering schools, some of them well equipped with apparatus, but usually unable to afford the labor necessary in carrying out extended investigations. Laboratories for testing cement upon engineering works are also common, but few of them have sufficient equipment for purposes of investigation, but some permanent ones might do much in this direction without material expense, by adopting a systematic plan.

Investigations upon cement usually require many experiments and need to be extended over long periods of time.

Many data have recently accumulated, throwing light upon many difficult points, but the results in many instances seem conflicting and involve the question in much confusion. The tendency to make a few experiments and draw general conclusions from insufficient data has been very marked in this line of work. Failure to investigate fully the character of the material used, or to note clearly the conditions under which the experiments are made, causes much confusion in comparing work of different investigators. Every detail of manipulation is important, and great care and accuracy are essential to good results.

CALCULATION OF THE ENERGY LOSS IN ARMATURE CORES. By Prof. W. E. GOLDSBOROUGH, Purdue University, Lafayette, Indiana.

THE paper discusses the character of the internal distribution of the magnetic flux in laminated iron cores. Experimental results are cited to show that the flux density, in a section of the core taken midway between the poles and parallel to the shaft, is much greater near the airgap surface than near the inner periphery. When the armature is in motion this lack of even distribution is intensified, owing to the repelling force exerted by the eddy currents, magnetic sluggishness in the iron and the disturbing reaction of the armature currents.

The experimental results were obtained by threading insulated wires through a series of holes drilled in a laminated iron core. These served as exploring coils.

By taking the uneven distribution into account, the calculated core losses agree with experimental determinations without the introduction of arbitrary constants into the formulæ.

[This paper will be printed in the Electrical World.]

FLUE GAS ANALYSIS IN BOILER TESTS. By Prof. D. S. JACOBUS, Stevens Institute, Hoboken, N. J.

THE paper, which the author intends to publish in full in the Transactions of the American Society of Mechanical Engineers, will contain a comparison of the results obtained by means of the Elliott gas analysis apparatus for the composition of gas with the composition as determined by mixing together known volumes of the elementary gases. In addition to this, the errors involved by the absorption of the gases in the water used in the apparatus and in the solutions will be discussed.

Experiments already made have shown that a large error may arise in measuring the initial volume of the gas on account of the absorptive power of the water for carbonic acid gas. To eliminate this error, we pass a considerable volume of gas through the measuring tube, allowing it to bubble up through the water in the bottle which forms part of the apparatus. This causes the water which clings to the sides of the glass measuring tube to become saturated with gas, so that there is no absorption before recording the initial volume; whereas, if the tube is simply filled with gas without forcing an excess of gas through it, there may be a considerable amount of carbonic acid gas absorbed before the initial reading is obtained.

To obtain an average sample of the gas over any desired interval of time, we use collecting bottles which we shall designate A and B.

The bottle A is completely filled with water, and the bottle B is filled to just above the point at which the rubber tube is connected at the bottom. Gas is drawn into the bottle A through the tube leading to the flue or chimney. When gas is drawn into the bottle A, the bottle B is lowered. The rate of flow of gas into the bottle A is regulated by adjusting the rate of flow of water from the bottle A to the bottle B by means of a glass cock provided therefor. This method of regulating the rate of flow is preferable to throttling the gas by means of the inlet cock to the bottle A because, if the gas is throttled on entering A, the sample collected in A will contain less carbonic acid gas than it should to represent a true average by volume. The deficiency in carbonic acid gas, if the gas is throttled on entering A, is caused by the retardation of flow through an increase of density which exists at the times when the percentage of carbonic acid gas is the greatest. After filling A the inlet cock is closed and the bottle B raised and placed on a shelf some height above A. The gas, which is then under pressure, is then led through a rubber tube to the Elliott apparatus.

The bottle B is lowered about five feet below the bottle A in order to cause the amount of suction, and consequently the rate of flow of water from the bottle A, to vary only between small limits. If desired, the bottle B can be lowered as the bottle A is being filled with gas, in order to obtain a more uniform flow of water from A than would exist with the bottle B stationary.

The flow from A to B varies as the square root of the head; hence a slight difference in the head of water will not cause a great variation in the rate of flow.

To allow for the absorption of gas in the collecting bottles, we draw the bottle A about one-half full of gas a number of times before starting the tests and shake the water it contains thoroughly, so as to cause it to become saturated with the gas.

Experiments have been made by adding salt to the water to decrease the absorptive power, and it is proposed to try other liquids than water. The results of all these experiments will be given in full in the paper.

STRENGTH OF ALUMINIUM ZINC AND ALUMINIUM TIN COPPER ALLOYS. By Prof. R. C. CARPENTER, Cornell University, Ithaca, N. Y.

[*Communicated by the Secretary.*]

THE paper describes the method of making the above alloys with different proportions of the various component parts, and also gives the strengths of various alloys; these are shown by tables and also by a curve. The aluminium zinc alloy has a maximum strength when it contains 33 per cent zinc and 67 per cent aluminium. Its strength is about 1½ that of pure aluminium and ten times that of zinc. This alloy is an exceedingly useful one and it can be used as a substitute for brass in nearly all kinds of construction. It melts at about 1000 Fahrenheit, it casts easily and readily and has a specific gravity slightly over three. On account of its low specific gravity it is much cheaper than brass, and on account of its excellent working qualities it will be found a very valuable alloy for many purposes. This alloy has been used extensively in the construction of models in the Department of Marine Engineering at Sibley College.

The aluminium, tin and copper alloys show even a greater variation in strength than the aluminium zinc alloy. In this case the alloy of maximum strength has a tensile strength of 63,000 pounds per square inch, while its constituents have strengths respectively 25,800, 15,000 and 5,000 pounds per square inch. The composition of the alloy of maximum strength is 87½ per cent copper, 6¼ per cent aluminium, and 6¼ per cent tin.

Another useful alloy which was composed of 80 per cent aluminium, 10 per cent tin, 10 per cent copper, has a tensile strength of 21,140 pounds, or about the same as that of cast iron and a specific gravity of three.

Both of the above alloys are readily made, easily worked and should doubtless prove of considerable value in the arts, but all other combination of aluminium, tin and copper are inferior.

INVESTIGATION OF THE STRENGTH OF WROUGHT IRON AT LOW TEMPERATURES. By Prof. R. C. CARPENTER, Cornell University, Ithaca, N. Y.

[*Communicated by the Secretary.*]

THE investigation described in the paper was undertaken for the purpose of determining the strength and other properties of wrought iron

at low temperatures. It describes the method in detail of making the test and also the results. The tests were made on an Emery Testing Machine of 200,000 pounds capacity, and were conducted through a range of temperature from 70 degrees above zero to 70 degrees below. The general results of the test show a total increase in strength with decrease in temperature amounting to 20 per cent of the strength at 70 degrees.

Results of previous tests made at Sibley College, with wrought iron at a high temperature, are also given for the sake of comparison. These results show an increase in strength up to a temperature of about 530 degrees F., after which a rapid diminution of strength occurs.

The investigation serves to show that the effect of decreasing temperature is to increase the strength and to raise the elastic limit without sensibly affecting the modulus of elasticity, or the elongation. The investigation also tends to show that a low temperature does not tend to increase the brittleness of wrought iron.

DEVELOPMENT OF ENGINEERING INDUSTRIES BY SCIENTIFIC RESEARCH. By Prof. WM. S. ALDRICH, West Virginia University, Morgantown, W. Va.

INDUSTRIES dependent upon an application of engineering science can hope to maintain their present high standard of excellence or further advance their interests only by the application of scientific methods in the development of engineering research along their respective lines. The research laboratory is as indispensable as the testing laboratory to the modern well-equipped engineering establishment; their functions are quite separate and distinct. In the development of new engineering industries the research is even more important than the testing laboratory.

Earlier engineering researches were largely carried on by individual scientists and engineers, by research committees of engineering societies, by consulting engineers, testing bureaus and inspection companies, by government experimental boards, by manufacturing establishments and corporations; and of recent date the engineering educational institutions have entered the field and specifically endowed institutions have been organized abroad for the prosecution of scientific and technical research. Organized efforts with trained staff and fully-equipped laboratories are quite as essential in research work as in manufacturing. Research must be made a business, not a passing pleasure.

[This paper will be printed in Journal of Franklin Institute.]

A NEW APPARATUS FOR TESTING INDICATOR SPRINGS. By Prof. M. E. COOLEY, University of Michigan, Ann Arbor, Mich.

ANY type of steam indicator is attached to a small vessel through which steam or other fluid at any temperature is passed, the pressure being reg-

ulated by suitable valves. In the bottom of this vessel a plunger is fitted which transmits the pressure in the vessel to a platform scale. The plunger has connected with it a heavy wheel and is fitted with ball bearings so as to rotate without sensible friction. During the test this plunger is caused to rotate either by spinning the heavy wheel or by means of a cord drive. This rotation does away with the friction and enables the pressure in the cylinder to be weighed with much accuracy. The apparatus is adapted to test springs in tension as well as in compression. For the tension tests the vessel is connected with a condenser. By alternately opening and closing the valves, the indicator is placed under the conditions existing on a low pressure cylinder. The plunger, together with its heavy wheel, is lifted to the extent of the vacuum and the reduced weight weighed on the scale the same as before. The apparatus has been used in the Mechanical Laboratory at the University of Michigan during the past year with very satisfactory results.

A NEW FORMULA FOR LEATHER BELTING. By Prof. J. J. FLATHER, Purdue University, Lafayette, Ind.

IN this paper a formula is presented for obtaining the width of leather belting when the average conditions under which it is to work are known. The ordinary formula $b = c \times \frac{HP}{V}$, in which C has values varying from 500 to 1100 for single belting, is not generally applicable to varying conditions and the author presents a formula which allows the width of belt to vary with a number of factors, all of which are either known or can be readily determined from the given conditions.

The loss due to centrifugal effect at high speeds and the influence of the arc of contact are recognized in many accepted rules for belting, and in addition to these the author takes into account the diameter of pulley when the latter is small, and also the character of joint whether laced or cemented splice.

The formula presented is

$$b = CC' \; C''k_0 \frac{HP}{V},$$

in which the values of CC' C'' and k_0 are given in tables which accompany the paper.[1]

In determining the value of C for single belting, the thickness was assumed at 0.20 inch; its heaviness 0.41 pound per square inch section, one foot long; the allowable working stress 350 lbs. for cemented joints (per square inch) and three-fourths of this, or 265 pounds, for laced joints.

[1] See also Western Electrician, June 12, 1897, for mathematical investigation of the formula.

The coëfficient of friction was taken equal to 0.27 for small slip and ordinary good belting.

In this determination the pulleys were assumed of approximately the same diameter, which was not less than fifty times the thickness of belt. C, under these conditions, was found to equal 800 for cemented and 1050 for laced joints.

C' varies from 1.4 for double belts running over an 8″ pulley to 1.10 for same, when used with a 20″ pulley. C'' varies from 1.83 with least arc of contact of only 120°, to 0.75 for similar arc of 240°. k_0 is not to be considered for speeds less than 2000 feet per minute. For 2500 feet $k_0=1.06$ for cemented joint, and for a speed of 5000 feet $k_0=1.34$.

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ADDRESS

BY

I. C. WHITE

VICE PRESIDENT, AND CHAIRMAN OF SECTION E.

THE PITTSBURG COAL BED.

Among all the rich mineral deposits of the great Appalachian field, the Pittsburg coal bed stands preëminent. Other coal beds may cover a wider area, or extend with greater persistence, but none surpass the Pittsburg seam in economic importance and value. It was well named by Rogers (H. D) and his able assistants of the 1st Geological Survey of Pennsylvania, in honor of the city to whose industrial growth and supremacy it has contributed so much. Whether or not the prophetic eye of that able geologist ever comprehended fully the part which this coal bed was to play in the future history of the city which gave it a name we do not know; but certain it is that the seven feet of fossil fuel which in Rogers' time circled in a long black band around the hills and, overlooking the site of Pittsburg from an elevation of four hundred feet above the waters of the Allegheny and Monongahela, extended up the latter stream in an unbroken sheet for a distance of two hundred miles, has been the most potent factor in that wonderful modern growth which has made the Pittsburg district the manufacturing centre of America, and which bids fair to continue until it shall surpass every other district in the world, even if it does not now hold such primacy.

That this claim for Pittsburg's supremacy is valid can hardly be doubted when we see its iron, steel, glass and other products going to every part of the Western continent and even invading the long established dynasties of the Old World. A brief account of the

main characteristics of such an important member of the Carboniferous series can hardly fail to be of some interest to geologists and others who desire to learn more of this celebrated coal bed and hence it has been chosen as my theme.

AGE.

The stratigraphical position of the Pittsburg coal bed is at the base of the Monongahela river series of Rogers. The thickness of this series varies from two hundred and fifty to four hundred feet in different portions of the Appalachian field. It also includes four other coal beds interstratified with sandstones, limestones and shales; but none of these coals have much economic importance since all are thin and impure except over quite limited areas, so that the Pittsburg bed may be regarded as the last of the great coal-making epochs of Carboniferous time.

The lower and middle Carboniferous had passed; the animals and most of the plants that characterize them had vanished; the great *Lepidodendra*, *Sigillariæ* and *Calamites* of the former floras had been succeeded by dwarfed and puny species of their tribe, while the tree ferns alone of all the larger plants appear to have flourished and attained considerable size. The evening of the Carboniferous day was well advanced, since marine conditions in the Appalachian field had terminated and brackish or fresh water conditions had arisen which continued to the close of the Permian. At the end of this latter epoch fifteen hundred feet of sediments had accumulated above the Pittsburg coal + the thickness eroded since the close of the Palæozoic, which most probably represents a much greater thickness of rocks than that (1500′) remaining.

Professor Fontaine and myself have shown (Report PP, 2nd Geological Survey of Pennsylvania) that, beginning with the horizon of the Waynesburg coal at say about 350 feet above the Pittsburg bed, the rocks contain a well defined Permian flora, of types common alike to the Permian of Europe and to the well recognized Permian beds of Texas (Bulletin G. S. A., Vol. 3, pages 117, 118, 1892). Just where in the series this flora was introduced we do not yet know, because no systematic collections of fossil plants have been made between the Waynesburg and Pittsburg coals, and in fact none until we pass below the Pittsburg seam several hundred feet, and reach marine conditions. The coal-making epoch of the Appalachian Carboniferous really culminated and its decline began with the deposition of the Upper Freeport bed at the summit of the

Allegheny river series of Rogers (No. XIII), since the few fossil plants found in the 600 feet of the Barren or Elk river strata, which supervene between the Upper Freeport and Pittsburg coals, are either identical with or closely affiliated to Coal Measure types of plants that survive into the Permian flora of Europe and Texas. This is also mainly true of the last marine faunal types occurring at the horizon of the Crinoidal limestone, about 300 feet below the Pittsburg bed, and therefore in Bulletin 65, U. S. G. Survey, page 19, the dividing line between the Upper and Middle Carboniferous was drawn through the midst of the Barren Measures (No. XIV), at the close of the Crinoidal limestone stage when marine life became practically extinct in the Appalachian sediments. Hence the 600 to 700 feet of strata extending from the Crinoidal limestone to the Waynesburg coal, and enclosing the great Pittsburg bed near the centre, may be considered as of Permo-Carboniferous Age, or so far as there is any evidence to the contrary, they could just as well be classed as Permian.

The flora of this portion of the column has been studied to only a limited extent; but, so far as known, it consists as already stated, mainly of those Coal Measure types which pass on up into the undoubted Permian, while the fauna comprises only fresh or brackish water forms, concerning which little or nothing is known, as the fossils (mostly minute) have never been studied. The rocks themselves consist of a monotonous succession of red shales, gray sandstones, and limestones, often highly magnesian but only slightly gypsiferous, and presenting much the same lithological appearance from the Crinoidal limestone to the top of the Permian, 1,500 feet above the Pittsburg coal.

The *Neuropteris morii* Lx., and the large reptilian tracks found by Lyell, near Greensburg, Pennsylvania, point to the same conclusion with reference to the age of the Pittsburg bed, namely, that it belongs to the closing stage of the Carboniferous period, rather than to the middle of the same.

AREA.

Before the drill of the petroleum seeker had penetrated every region of the great Appalachian basin, it was supposed that the Pittsburg coal spread in a continuous sheet under every portion of that area where its outcrop was buried from view. This conclusion was based upon the unfailing continuity of the bed southward for 200 miles from Pittsburg to the headwaters of the Mononga-

hela, and also westward into Ohio, and its reappearance on the river of that name at Pomeroy, as also on the Great Kanawha at Raymond City, Pocatallico and Charleston But the studies of Professor Orton and others in Ohio, and my own in West Virginia, aided by the petroleum drilling there, have shown that the coal is absent or but poorly developed over large areas where it had formerly been considered present. Hence to the list of counties of West Virginia named in Bulletin 65, U. S. G. Survey, page 64, where this coal is absent or in poor development, must now be added Doddridge, Tyler, and probably half of Wetzel, since two tests with the diamond drill, near the centre of the latter county, found only two feet of coal at a depth of 425 feet below the valley of Fishing Creek. This area, together with that previously known to be barren or to have only a patchy development in West Virginia and Ohio, will aggregate between 4,000 and 5,000 square miles, a rather startling figure when subtracted from the supposed area of a coal bed so valuable as the Pittsburg in its developed regions.

There has been much speculation as to the area which this coal may once have covered. The isolated patches of the bed in the Georges Creek and North Potomac regions, the few knobs of it in Preston, Barbour and Upshur counties of West Virginia, together with its presence in the solitary peak of Round Top in Bedford county, Pennsylvania, forty five miles from any other outcrop of the bed, and far east of the Alleghany mountains, have led many geologists to believe that the Appalachian Coal Measures may once have extended northwestward to the Lake region and eastward possibly to the North Mountains or even to the Blue Ridge, having been removed from all this wide expanse by the enormous erosion to which it has been subjected since Carboniferous time. Whether the limits thus assigned were ever attained by the spread of the Coal Measures we shall probably never know to a certainty, but that there is no inherent improbability in the hypothesis will appear from the fact that the oldest member of the Carboniferous period, the very hard and erosion resisting sandstones of the Pocono, with its included coal-beds extends to the North Mountain region at several points along that great ridge. Of course if the Coal Measures ever covered an area as wide as this lowest member of the Carboniferous, the probabilities are that the area of the Pittsburg bed which has escaped erosion is only a fragment of its former extent. But however this may be, its entire area of work-

able coal remaining in the states of Pennsylvania, Ohio, West Virginia and Maryland, does not probably exceed 6,000 or 7,000 square miles.

STRUCTURE.

Dr. J. J. Stevenson, of the University of New York, was the first geologist to make a detailed study of the Pittsburg coal bed and to describe the peculiar structure which so distinctly characterizes it that the coal seam may thereby be identified with great certainty over a wide area. In Report K, 2nd Geological Survey of Pennsylvania, he shows that a series of thin parting slates and clays subdivide the bed into several definite members which may be grouped as follows:

"Roof" coals.
"Over"-clay.
"Breast" coal.
  Parting.
"Bearing-in" coal.
  Parting.
"Brick" coal.
  Parting.
"Bottom" coal.

The "Roof" coals are a number of thin layers of coal (2 to 12 inches each) separated by shales or clays of varying thickness. Some of the layers are good coal, while others contain much dirt and other impurities. Their number ranges from 1 to 8, or even more, and their combined thickness seldom exceeds 3½ to 4 feet, while the separating slates and clays may be only half as much, or they may often exceed the coal in thickness by two or three times. In practical mining operations all of this "Roof" coal is wasted, because the coal layers make a good support for the overlying strata and are therefore left as the roof of the mine. In this way about 2,000 tons per acre of the Pittsburg coal are always lost without any attempt to recover it. This waste is so large that some of the Mining Companies are considering the question of putting in crushing and washing machinery with a view to taking down these roof coals and thus preventing the great loss of fuel which their abandonment entails upon any mine. There is no doubt that the time will come many generations hence when, at great cost, the Pittsburg bed will be re-mined to secure the coal which is now

rejected both in its roof and bottom members, since all of it would be valuable fuel if freed from the included slates and clays.

The "Over"-clay is an impure fire clay, and varies much in thickness, sometimes almost disappearing and again thickening up to 2 or even 5 feet. The clay is usually mottled and much slickensided, so that it becomes a dangerous trap when left as a mine support, since large pieces of it will drop from the roof without any warning sound. Hence it is generally taken down at once and the miner has therefore given it the name of "Draw-slate" in many regions. It often contains what appear to be stems and rootlets of plants.

The next succeeding (downward) division of this seam, the "Breast," coal of the miners, also often termed the "Main Bench," is the most important and valuable division of the whole bed. Its thickness gradually increases from the Pittsburg region (where it is usually about 3 feet) up the Monongahela, attaining a maximum of 6 feet at Brownsville, while to the eastward in the Georges Creek and North Potomac basin of Maryland and West Virginia, it increases still more to 7½ or even 10 feet. The top of this member is nearly always of a bony nature for a thickness of 1 to 4 inches, and frequently this must be separated and rejected in mining, but even where this is not required, the top of the "Breast" coal is distinctly harder than the rest of it and inclined to a cannelly structure. Westward to the Ohio river this "Breast" division thins and in the Glendale and Moundsville shafts is only 21 inches, according to Mr. J. W. Paul, State Mining Inspector of West Virginia. It is still perfectly distinct, however, with the twin slates, ¼ inch thick each, and enclosing 6 inches of "Bearing-in" coal immediately below.

The "Bearing-in" coal is so named by the Monongahela river miner because, in mining operations, the under-cutting of the "Breast" coal is made in this layer, the latter being then wedged or blown down and the "Brick" division subsequently taken up. The "Bearing-in" coal is usually brilliant and pure, varying in thickness from 3 to 6 inches, and enclosed by two thin parting slates, so much alike in color and structure as to be almost indistinguishable. Their color is usually a dark, mottled gray, and they vary in thickness from ¼ to 1 inch. The persistency of these twin slates over all the regions drained by the Monongahela and east to the Georges Creek and North Potomac field, while westward to Wheeling, Bellaire and the neighboring regions of Ohio, they still

appear to be present, is one of the remarkable features of this coal bed. When, however, the areas of this coal south of the little Kanawha river in West Virginia, and west from the Muskingum in Ohio, are examined, these twin slates are not found; or, if represented, are no longer recognizable as the Monongahela partings, but the "Roof" coals and "Over"-clay appear to be present.

The "Brick" coal layer comes next under the lower of the twin slates and was so named by the Monongahela river miners because it comes out in oblong, rectangular blocks resembling the shape of common bricks. It is usually about 1 foot thick. The parting which separates the "Brick" coal from the next lower member is always present along the Monongahela from Brownsville to Pittsburg and it is also represented in the Georges Creek and North Potomac field, but in the Fairmont region it is only occasionally present, the bed there being generally undivided below the "Bearing-in" coal.

The "Bottom" member is from 12 to 20 inches thick along the Monongahela and contains so many thin, slaty, sulphurous laminæ, that it is usually not taken out in mining, and thus another thousand tons per acre of this bed are wasted, though in the Fairmont and Cumberland (Georges Creek) regions it is mined and sold with the rest of the coal. The 12 to 15 inches of good fuel in this member could always be recovered by crushing and washing.

The structure here described can be best illustrated by giving an actual section of the coal at its type locality. In the Ormsby mine at 21st Street, Pittsburg, where mining operations have been carried on for more than sixty years, Mr. J. Sutton Wall took the following measurements (K 4, 2nd Geological Survey Pa., page 177):

		Inches.	
"Roof"	Coal	6	56″
	Clay	2	
	Coal	8½	
	Parting	0½	
	Coal	2	
	Clay	9	
	Coal	8	
	Parting	0½	
	Coal	9	
	Clay	0½	
	Coal	5	
	Parting	0½	
	Coal	2	
	Parting	0½	
	Coal	2	

"Over"–clay	9		
"Breast" Coal	33	70¾′	
Parting	0¼		
"Bearing-in" Coal	4		
Parting	0¼		
"Brick" coal	10		
Parting	0¼		
"Bottom" coal	14		
Total thickness			10′ 6¾″

Substantially this structure may be seen at every mine between Pittsburg and Brownsville and on beyond for many miles (see Reports K and K4, 2nd Geological Survey, Pa.).

East of the Monongahela, on the Youghiougheny river, the same structure is well illustrated by two sections which Mr. W. S. Gresley, F. G. S. A., measured for me with great care at the W. L. Scott estate mines of which Mr. Gresley is superintendent at Scott Haven, Pennsylvania. The first one of these is near Scott Haven and reads as follows:

		Inches.	
"Roof"	Coal, several films of dirt	3¼	46¼″
	Shale, black earthy	2	
	Coal	2¾	
	Shale, gray, streaks of coal near top	11	
	Bone (hard, dull, impure, coaly layer)	1	
	Coal	2½	
	Shale, black	0¼	
	Coal	1¾	
	Shale, black, coaly	1	
	Coal	3	
	Slate gray, with irregular coal streaks	4½	
	Coal, compact, free from "binders"	9¼	
	Slate, with coal streaks	1½	
	Coal	2½	

"Over"-clay (impure, fireclay, light gray above, getting browner and then a much darker gray with coal streaks of irregular shapes, especially towards base)	10¼′

"Breast" coal (with 1½ inches of bone at top, and next 10″ harder than rest of bench)	41½	72⅝″	
Shale, dark grayish brown, mottled	0¾		
"Bearing-in" coal, clear and brilliant	4		
Shale, dark, grayish brown, mottled,	0½		
"Brick" coal, clear and brilliant	11		
Shale, parting	0⅛		
"Bottom" coal { coal with a few thin dirt layers	12½		
shale	0¼		
coal, bright, clean	2		
Total thickness of bed			10′ 9⅝

The other section made by Mr. Gresley is from the "Pacific Mine," near Scott Haven and three miles distant from the section just given. It is as follows:

		Inches.	
"Roof"	Coal	1½	50″
	Shale, light	2¼	
	Coal, with a few dirt partings	5	
	Shale	0½	
	Coal	2	
	Fireclay, light, bastard	14½	
	Coal, with a few dirt partings	9	
	Shale	1½	
	Coal, with thin dirt lenses	12	
	Shale	0¼	
	Coal	1¾	

"Over"-clay (fireclay, light, inferior, much darker toward base with meandering streaks and veins of brilliant coal) . . . 10½″

"Breast" coal, upper 10 inches harder than the rest	42	71⅞″	
Shale, mottled	0½		
"Bearing-in" coal	3¾		
Shale, mottled	0½		
"Brick" coal	11		
Shale, parting	0⅛		
"Bottom" coal	14		
Total thickness of bed			11′ ⅜″

A third section measured by Mr. Gresley three miles distant from either of these differs so little from them that it is useless to give it.

How perfectly this great coal bed preserves the Pittsburg type of structure, is shown from the following section sent me by Mr. R. L. Somerville, superintendent of the Georges Creek Coal & Iron Co., Lonaconing, Maryland. The locality is east of the Alleghany Mountains, and 150 miles from Pittsburg. It is as follows:

	Inches.	
"Roof" coal with slate parting below	20	
"Breast" coal 6″ of bone on top	91	
Slate	1	
"Bearing in" coal	4½	
Slate	0¾	
"Brick" coal	16	
Slate	0¼	
"Bottom" coal	15	
Total thickness of bed		12′ 4½″

This type of structure is practically universal over all of the Pennsylvania, Maryland and Eastern Ohio area of the bed. The different members vary considerably in thickness, as, for instance, the gradual increase of the "Breast" coal from 3 feet at Pittsburg to 6 at Brownsville, fifty-eight miles up the Monongahela river, or to 7 and even 10 feet in the Georges Creek and North Potomac regions of Maryland and West Virginia, or a decrease may take place in the same to 30 and sometimes to 20 inches as in the Wheeling and Bellaire regions; but each of the main subdivisions can be distinctly recognized so that, whether at Fairfax Knob, on the summit of the Alleghany Mountains 3200 feet above the sea, or deep down in the centre of the great Appalachian trough buried under fifteen hundred feet of sediments, the explorer can readily identify this great coal bed, not only from its associated rocks but from its stratigraphical elements as well, and often from even the fracture of the coal. I once had a practical illustration of this latter peculiarity of the Pittsburg seam. About the year 1880 a coal bed was discovered near the summits of the hills, south from Huntington, West Virginia, and on one of my excursions to the southern portion of the State with the University students of Geology, the mayor of Huntington requested me to determine, if possible, to what horizon the coal belonged. It proved an easy problem to identify it since the Crinoidal limestone with its characteristic fossils was easily found in the bed of Four Pole Creek, fifty feet above the Ohio, and above it the ordinary rock succession

of the Barren or Elk River series. But being anxious to know what the miner, who was digging the coal, thought of the matter he was interrogated and replied as follows: "I don't know anything about geology, but I dug coal several years in the Pittsburg seam along the Monongahela and this coal reminds me of the Pittsburg in the way it breaks into blocks." Thus had the miner correctly diagnosed the horizon of the bed by his own peculiar methods, though three hundred miles distant from where he had learned its structure, with only the tools of his trade and his bright observing mind as his guidance, strong testimony certainly to the persistence of even the internal structure of the bed.

The oil-well driller is required to identify this coal correctly in the great petroleum districts of West Virginia and Pennsylvania, between the Ohio and the Monongahela rivers, where it is buried from sight by the Permian beds all the way from 500 to 1,500 feet. It is there a key-rock for determining the amount of casing and the depth to the oil sands, and thus many dollars of expense depend upon the correctness of the driller's identification. This he does by observing the character of the drillings as brought to the surface by the sand pump; or, in other words, he observes the stratigraphical succession in his own peculiar way, and in the hundreds and even thousands of holes drilled in this area, he has only two or three mistakes charged against his accuracy of discrimination.

A word of friendly criticism and kindly warning concerning the methods of the U. S. Geological Survey, especially in its Coal Measures work, but equally applicable to the other formations, becomes in this connection an imperative duty.

In recent years a theory seems to have been adopted by the U. S. Geologists who have been studying the Coal Measures that no coal bed can be certainly identified beyond the area of its continuous outcrop, and hence must be given a local name for every isolated area, thus adding greatly to the burden of geological nomenclature, a fault of geologists everywhere, which has become so grievous that the International Congress has been invoked this summer to consider a remedy for the matter. The confusion produced by this useless giving of many names to the same thing is an evil for which a remedy must be speedily found, or it will soon bring all geological work into deserved contempt in the minds of laymen.

The United States Geological Survey, which is doing such splendid work along many lines, ought to be a model in the matter

referred to, but is now the chief offender. Let us hope and urge that a reform in the methods of work which lead to such undesirable results will soon be inaugurated.

The old and well established names of the New York, Pennsylvania and Virginia Surveys, rendered classic by the labors of such men as Hall, Emmons, the Rogers Brothers, Lesley and many other faithful geologists, should not be lightly cast aside and the work of these noble pioneers ignored unless positive error can be proven.

It is no argument in favor of the methods complained of to say that the geologist is not reasonably certain of identity of horizon, for that is the fault of the observer and his methods in not wisely attacking the problems of stratigraphy. It will hardly do to admit that the untutored miner and unlettered petroleum driller are better geologists than men trained as experts in geology. What we need more than anything else is a closer and more minute study of the individual beds, such as Mr. Gresley, for instance, has been making on the Pittsburg coal, and if this method of work were pursued the geologist would find but slight need of the introduction of new names for old and well-named things. It was with the hope of emphasizing the necessity and importance of observing the smaller details of stratigraphy more closely, that I have dwelt at length upon the characteristic structure of a single coal bed.

PAPERS READ.

[ABSTRACTS.]

STYLOLITES. By T. C. HOPKINS, State College, Centre Co., Pa.

[This paper is printed in the American Journal of Science, Aug., 1897.]

A SUGGESTION IN REGARD TO THE THEORY OF VOLCANOES. By Prof. WILLIAM NORTH RICE, Middletown, Conn.

ALL writers since Scrope have recognized the important rôle of steam in volcanic eruptions; but Prestwich is doubtless right in maintaining that steam cannot be considered the primary cause. There is an uninterrupted series of gradations between explosive eruptions, like that of Krakatoa, and outpourings of lava with little or no explosive action, as in the northwestern United States. There must be a common cause for all types of eruption.

Prestwich conceives eruption to be due to squeezing out of material of subcrustal liquid layer by contraction of earth. But crust contracts less than interior, so there can be no squeezing-out of contents.

Distribution of volcanoes affords clue to cause. The following general laws of distribution of volcanoes have been noted: — They occur in mountain ranges, and (as remarked by Suess) in ranges of the Cordilleran type (anticlinoria of Dana) more abundantly than in ranges of the Appalachian type (synclinoria of Dana); they occur near the ocean; they occur in regions of recent strata; they occur in connection with fringing reefs, and not in connection with barrier reefs (Darwin). These laws suggest a more comprehensive generalization — volcanoes occur in localities where there has been recent elevation. See map in Berghaus' Physikalischer Atlas, showing relation of volcanoes to seaward migration of shore lines.

Interior of earth is probably solid, but at no great depth the material is potentially liquid (potentiell magmatisch, Reyer). Local elevation of crust relieves pressure, and allows more or less liquefaction (melting point being lowered by relief of pressure). Lava expands in melting, and ascends through all fissures that may be opened. If fissures are freely opened, there may be non-explosive eruptions. Ascent of lava may cause aqueo-igneous fusion of sedimentary rocks, and magma may become

more highly charged with steam. Sudden vaporization of water in subterranean reservoirs may account for explosions of extreme violence, as in Krakatoa.

King and Reyer have considered relief of pressure as cause of eruption. but King attributes relief of pressure to erosion, and Reyer to formation of fissures. A suggestion that crustal uplift is cause of eruption is given by Geikie; but I have believed that the idea deserves somewhat more explicit formulation.

An objection to the view here presented seems to be found in the occurrence of contemporaneous sheets of igneous rock interstratified with thick masses of sediment. Thick sediments of course show subsidence. Two answers may be made to this objection: (1) The liquid rock may come from regions of elevation adjacent to the subsiding troughs or basins. (2) The sheets of igneous rock may mark oscillations in the movement, the progressive subsidence being interrupted by epochs of elevation.

THE ORES AND MINERALS OF CRIPPLE CREEK, COL. By H. P. PARMELEE, Charlevoix, Mich.

OBSERVATIONS ON THE GENUS BARRETTIA. By R. P. WHITFIELD, American Museum of Natural History, New York City.

THE author refers to the original discovery of the fossils and description of the genus by S. P. Woodward, with its reference to the Rudestidæ. Then points out several peculiar features of the fossil not noticed in the original description and points out their strong resemblance to cup corals, and their general radiate structure, based upon a large collection of specimens obtained from Jamaica, W. I.

[This paper will be printed in The Bulletin of the American Museum of Natural History, Vol. IX, pp. 233–246, plates, XXVII–XXXVIII.]

LAKE CHICAGO AND THE CHICAGO OUTLET. By FRANK LEVERETT, U. S. Geological Survey, Denmark, Iowa.

[This paper is printed in Bulletin No. 2 of the Geological and Natural History Survey of the Chicago Academy of Sciences.]

RECENT EARTH MOVEMENT IN THE GREAT LAKE REGION. By G. K. GILBERT, U. S. Geological Survey, Washington, D. C.

[This paper will be printed in the 18th Annual Report of the U. S. Geological Survey.]

THE LOWER ABANDONED BEACHES OF SOUTHEASTERN MICHIGAN. By F. B. TAYLOR, Fort Wayne, Ind.

[This paper will be printed in the American Geologist]

SOME FEATURES OF THE RECENT GEOLOGY AROUND DETROIT. By F. B. TAYLOR, Fort Wayne, Ind.

THE city of Detroit is built largely upon one of the terminal moraines laid down by the retreating ice-sheet. But this moraine lacks the ordinary character of moraines laid down on land because it was deposited in nearly 200 feet of water. For while the ice-front stood at Detroit the western half of the basin of lake Erie was filled with a large glacial lake. It was the presence of this lake that caused the land in the St. Clair-Detroit valley to be so smooth and devoid of prominent surface features. This smoothness of surface extends up to about 200 feet, which was approximately the upper limit of the glacial waters.

The old shore lines and beaches of these glacial lakes are well developed near Detroit, but they have not been investigated by the author south of the vicinity of Port Huron.

One of the most interesting features in the vicinity of Detroit is found in the drowned condition of the lower courses of all the tributaries of the Detroit river. Even the smallest streams whose whole length is not over two miles have deep estuaries—much deeper than could have originated by their own erosion under present conditions. Many of these were navigable naturally and form excellent harbors for the smaller lake craft. There were several drowned streams originally where the city of Detroit now stands; among them were Parent's, Savoyard, May's. Knagg's and other creeks, while a little to the west of the city is the river Rouge, a larger stream which shows the effect of drowning for some eight or nine miles above its mouth. About three miles above its mouth the Rouge receives a tributary from the north called Baby's Creek. It is a very small stream, but it has a depth of eight or nine feet for nearly a mile from the Rouge, and it was here that Commodore Perry, after his great victory on Lake Erie in 1813, brought his fleet for repairs. Baby Creek itself has a small short tributary from the east which is navigable and a tract of land

bordering upon it was set apart at an early day for a ship yard. Another very small tributary of the Rouge about half a mile farther up, called Campbell's creek, shows drowning in a very marked way. The average mid-stream depth of the Rouge for at least four miles above its mouth is eighteen to twenty feet. This is perhaps three or four times the depth that would be expected from erosion of the stream itself. The Savoyard was where the busiest part of the city is now located. There was a bridge across it at Congress street where the depth of water was about ten feet. This creek was the harbor of early Detroit, but with the growth of the city it became an open sewer and grew so obnoxious that it was finally covered over. Most of the other estuaries mentioned within the limits of the city have been filled up.

The tributaries of the St. Clair river—Black, Pine and Belle rivers—and the tributaries of lake St. Clair, the Clinton and the Thames, show the effects of drowning very clearly. This peculiar condition of the tributaries is supposed to have come about in consequence of the following changes in the condition of the St. Clair-Detroit river system. First, these rivers existed for a considerable period nearly as they are to-day, differing only in the fact that they flowed at a slightly higher level. Then for another considerable period of time the three upper Great Lakes discharged their waters eastward to the Ottawa valley from the north end of Georgian bay, and during that time the bed of the St. Clair and Detroit rivers was abandoned. It was during this time that all the tributaries deepened their lower courses and cut them back into the plain of the valley floor. It seems probable that the tributaries collected in the bottom of the old abandoned beds and flowed through a series of ponds and marshes to one or the other of the adjacent lakes. During this time they were cutting down their beds to a base level twenty-five or thirty feet below the present surface of the river. Finally there came another change, an uplift at the northeast, which brought the water of the three upper lakes back to the abandoned beds and the present condition of things was then established. Naturally, when the water filled up the greater abandoned beds it also backed up into the deepened lower courses of the tributaries, and that is why they are all so deep to-day. The Rouge river shows this effect very clearly and Baby's and Campbell's creeks still better, and all these may be reached by a short walk from the end of the Fort street electric car line.

That the St. Clair and Detroit rivers abandoned their beds for a considerable time and then returned to them again is a conclusion which had been reached previously from the study of the upper lakes, and also from the study of the characters of the Niagara gorge which shows great changes of volume on the part of the river. The discovery of the drowned tributaries of the St. Clair and Detroit rivers confirms this idea quite strongly.

AN ACCOUNT OF THE RESEARCHES RELATING TO THE GREAT LAKES. By J. W. SPENCER, Ph.D., Washington, D. C.

CHANGES OF LEVEL IN THE GLACIAL FORMATIONS OF THE ALPS. By Prof. ALBRECHT PENCK, University of Vienna, Vienna, Austria.

THE interesting papers of Mr. Taylor, Mr. Gilbert and Mr. Spencer remind me of the efforts made in the Alps to determine if there have been earth movements connected with our lakes, and if the Section allows I will point out in a few minutes the results at which we arrived. Professor Heim has observed that there are on the shores of the Zurich Lake terraces, cut into the Molasse rock, which he assumes to be made by river action. Some of these terraces have a steeper slope than the lake valley and some have reverse slope, and dip against the Alps. Heim therefore assumes that the whole mountain chain, after having been formed by folding, has sunk down bodily, and that the slopes of its valleys have been altered. The upper parts are sunk below the lower, and transformed in this way into lakes. These movements are assumed to have taken place during the glacial epoch, for after its end the lakes were in existence.

When surveying the environs of the largest lakes of the Northern Alps, due attention was paid to the post-glacial terraces of the Lake of Constance. Dr. Sieger found no evidence that they have been disturbed. Everywhere around the lake they have the same height, with such exceptions as are due to local ice dammings, and there is no evidence on the north side of the Alps that the sinking of the whole range, which has been assumed by Heim, is still going on.

The detailed mapping of the Lake of Constance region proved other kinds of earth movements, affecting the glacial deposits.

The deposits belonging to the glacial period of the Alps must, as to their origin, be divided into true moraine and fluvio-glacial, the former being the true glacial deposits, the latter being deposited by glacial rivers. The moraine deposits occur everywhere where glaciers have existed, and are not confined to certain levels. The fluvio-glacial gravels, as river deposits, must have had originally a slope in the direction in which the river flowed. This slope has been in some places destroyed by earth movements. This can be shown in the best way by following the oldest of the three fluvio-glacial deposits, the high-level gravel. It formed originally an extensive covering of the low ground of the north Alpine Piedmont region, sloping to the north. Now it forms a series of very flat folds which run parallel to the foot of the western Alps. These folds can be followed from the mouth of the Aar into the Rhine, and from the mouth of the Lech into the Danube, that is for a distance of 250 miles. Their general arrangement is the following: One syncline runs along the foot of the Alps, another along the foot of the Jura Mountains; the country between them shows several anticlines and synclines. The difference of the heights of the synclines and anticlines amounts in several places to 200 and 300 feet.

On the shores of the Lake of Constance some irregularities occur in the strike of the folds. They curve toward the mountains, forming a V, which can be traced also in the structure of the Molasse and interior chains

on both sides of the Rhine valley, and forms here, as well as on both sides of the Arve, Rhone, Aar and Enns valleys, a very conspicuous feature in the structure of the Alps.

Of the younger fluvio-glacial formations, the higher and lower terraces, only the former show clear evidence of folding, and this is far less than that of the high-level gravel; but there are certain indications that even the lower terraces have been affected by folding. These indications are found in the different positions of the three fluvio-glacial formations in the different parts of the Alpine forelands. The regular succession is that the younger fill up valleys and cut into the older ones. Thus we have periods of deposition alternating with periods of erosion. But in some places the three fluvio-glacial deposits are superposed. Here only deposition occurred and no erosion in the interglacial times. This seems to be due to folding, which checked the erosion in the synclines. Indeed in the folded districts the work of erosion between the three gravel formations is far greater in the anticlines than in the synclines.

The earth movements of the glacial period in the Alps are of a different type from those about the grand American lakes. The American belong to a warping of the earth's crust, which is independent of structural lines, the sub-Alpine show that the folding of the Alpine system was still going on in early glacial times in the foreland. Whether or not this folding was connected, as Heim assumes, with a bodily sinking of the whole mountain chain, could not be ascertained.

A SUPPLEMENTARY HYPOTHESIS RESPECTING THE ORIGIN OF THE LOESS OF THE MISSISSIPPI VALLEY. By Prof. T. C. CHAMBERLIN, University of Chicago, Chicago, Ill.

THE distribution of the loess in the Mississippi Valley is very significant. (1) It is distributed along the leading valleys. (2) It thickens along the border of the Iowan ice sheet (the main loess deposit only being here considered). The loess is composed in part of particles of the decomposable silicates and carbonates, which abound in the glacial deposits but not in the residuary soils. These and other correlated facts indicate a genetic relationship to glacial and river action.

On the other hand, the purely aqueous theory encounters grave difficulties in the great vertical distribution of the loess, the overlap of summits, the lack of horizontality of border, or uniform gradient of border, the wide distribution of land shells, etc.

The supplementary hypothesis recognizes the great force of the glaciofluvial arguments and assumes their correctness for the primary deposits. It assumes the presence of an ice sheet and a low slope of the land, a consequent wide wandering of the glacial waters, and the deposit of the glacial grindings over broad flats. It assumes great fluctuations in the

glacial waters (1) as a result of periods of warm weather, and (2) of warm rains. The flats were, therefore, periodically flooded and left dry. In the latter state they were facily exposed to the sweeping of the winds and dust could be derived in great quantities from them to be borne away over the adjoining lands and lodged in its vegetation. The material would thus be essentially fresh glacial silt. The land mollusks of the uplands would not only be likely to be buried *in situ* but also to be washed down to the lowlands and buried there. Occasionally they would invade the lowlands between overflows and be caught there. In some cases the lowland and aqueous molluscan shells would be borne to the highlands by organic agencies and the severer winds.

To make this a good working hypothesis there must be an accommodation between the extent of fluvial and of eolian action respectively. If the eolian phase is pushed too far there is not sweeping ground enough to give the requisite silt, for it must be enough to overmatch erosion. If the fluvial hypothesis is pushed too far it does not leave feeding ground enough for the land mollusks.

Richtofen made the Chinese loess primarily eolian and secondarily fluvial and lacustrine. This hypothesis makes the Mississippian loess primarily glacio-fluvial and secondarily eolian.

[This paper will be printed in the Journal of Geology.]

WASATCH[1] AND BRIDGER BEDS IN THE HUERFANO LAKE BASIN. By Prof. HENRY FAIRFIELD OSBORN, Columbia University, New York.

IN 1888 Professor R. C. Hills, of Denver, announced his very important discovery of Tertiary beds in the Huerfano Basin of Southern Colorado. He contributed three papers to this subject in the Proceedings of the Colorado Scientific Society in 1888, 1889 and 1891, and finally divided the beds into three series, namely:

Huerfano Series (Eocene)	Huerfano Beds	Bridger Group	3,300
	Cuchara Beds	. . . .	300
	Poison Canon Beds	Lower Eocene (Green River Wasatch and Puerco) .	3,500

The identification of the Huerfano beds proper was made by means of a large collection of fragmentary fossils. The identification of the lower beds was upon stratigraphic evidence only, Professor Hills observing that they underlay conformably the upper beds. The essential features of his conclusions were as follows:

[1] Since this paper was read, the fauna proves to be Wind River, rather than Wasatch.

(1) That the Huerfano Series of 3,300 feet are equivalent to the Bridger or Middle Eocene, and the Cuchara and Poison Canon Series are probably equivalent to the Lower Eocene.

(2) At the close of the Laramie Period the great anticlinal axis was over to the east and southeast of the Wet Mountain Range, and east of Spanish Peaks, forming the eastern border of the lake, extending fifty miles north and south, and from ten to twenty miles east and west.

(3) The eruption of the laccolithic Silver Mountain and Spanish Peaks was subsequent to the Upper Lake deposits of Bridger age. Hence these deposits are found upon the slopes of Spanish Peaks.

(4) The drainage of the Huerfano Lake was to the north through the Wet Mountain Valley.

In May, 1897, the writer, accompanied by Dr. J. L. Wortman, made a brief reconnaissance of this basin, and came to the following conclusions, differing from those reached by Professor Hills:

(1) That west of the Huerfano canon the variegated marls, clays, soft shales and sands aggregate only 800 to 1000 feet in thickness, are nearly horizontal in position, and constitute alone the true Huerfano lake deposits. They may be positively divided into upper beds, equivalent to the Bridger. From the observations and conclusions made in the basin there are also undoubtedly lower beds, equivalent to the Wind River and Wasatch.

(2) That the so-called Cuchara and Poison Canon Beds are unconformable with the Huerfano and are of older age, probably cretaceous as partly determined by the presence of *Baculites* in the Poison Canon section, which was selected by Professor Hills as typical.

(3) That the eastern boundary of the Huerfano Lake is partly indicated in the present canon of the Huerfano River; that this boundary extended to the south so as to include the base of Silver Mountain toward the Cuchara divide; that it lies from three to seven miles west of the anticlinal axis described by Professor Hills, and that, therefore, the Huerfano Lake deposition did not extend as far to the east or south as the Spanish Peaks.

The geological features of these conclusions can hardly be dignified by the term "A theory of the Huerfano Lake," for they were formed during a hasty survey of this basin; while Professor Hills' results certainly deserve the deliberate consideration of a prolonged survey. In fact this basin with its volcanic disturbances and eruptions presents a fascinating problem in the geology of tertiary times. Among the Bridger forms discovered were many portions of the skeleton of *Tillotherium*, besides remains of *Hyrachyus*, *Palæosyops*, *Microsyops*, *Calamodon*, *Stypolophus* and *Pachynolophus*. This region is peculiar in the absence of *Uintatherium*. In the lower beds are found teeth and limb bones of *Coryphodon*, *Phenacodus*, *Oxyæna*, *Pantolestes* and other lower Eocene forms, probably of Wasatch age.

The writer is greatly indebted to Professor R. C. Hills for his very full information in regard to the topography of the basin, and for assistance and advice in connection with the trip.

LOWER CARBONIFEROUS OF HURON COUNTY, MICHIGAN. By ALFRED C. LANE, Assistant State Geologist, Houghton, Mich.

[*Communicated by the Secretary.*]

PROGRESS OF HYDROGRAPHIC INVESTIGATIONS BY THE U. S. GEOLOGICAL SURVEY. By F. H. NEWELL, U. S. Geological Survey, Washington, D. C.

ICE JAMS AND WHAT THEY ACCOMPLISH IN GEOLOGY. By M. A. VEEDER, M.D., Lyons, Wayne County, N. Y.

On TUESDAY the GEOLOGICAL SOCIETY OF AMERICA occupied the time of SECTION E, and the following papers were read.

THE GRANITE MOUNTAIN AREA OF BURNET COUNTY, TEXAS. By FREDERIC W. SIMONDS, Austin, Texas.

EXPOSURES NEAR DETROIT OF HELDERBERG LIMESTONES AND ASSOCIATED GYPSUM, SALT AND SANDSTONES. By W. H. SHERZER, Ypsilanti, Mich.

THE NOMENCLATURE OF THE CARBONIFEROUS FORMATIONS OF TEXAS. By ROBERT T. HILL, Washington, D. C. (Read by Title.)

STRATIGRAPHY AND STRUCTURE OF THE PUGET GROUP, WASHINGTON. By BAILEY WILLIS, Washington, D. C.

THE LOESS AS A LAND DEPOSIT. By J. U. UDDEN, Rock Island, Ill.

CLAY-VEINS VERTICALLY INTERSECTING COAL MEASURES. By W. S. GRESLEY, Erie, Pa. (Read by Title.)

ANALOGY BETWEEN DECLIVITIES OF LAND AND SUBMARINE VALLEYS. By J. W. SPENCER, Washington, D. C.

GREAT CHANGES OF LEVEL IN MEXICO AND THE INTER-OCEANIC CONNECTIONS. By J. W. SPENCER, Washington, D. C.

A Joint Session with Section H was held on Wednesday afternoon for the Discussion on the Implement-bearing beds of Trenton.

SECTION F.

ZOÖLOGY.

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WM. H. ASHMEAD, Washington, D. C.

Committee to Nominate Officers of Section.

The Vice President and Secretary; and CHARLES S. MINOT; THEODORE GILL; F. M. WEBSTER.

ADDRESS

BY

LELAND O. HOWARD,

VICE PRESIDENT, AND CHAIRMAN OF SECTION F.

THE SPREAD OF LAND SPECIES BY THE AGENCY OF MAN; WITH ESPECIAL REFERENCE TO INSECTS.

AMONG the many influences which during the last century or two have been affecting that unstable condition of life which is expressed in the words "the geographical distribution of animals and plants," none has approached in potency the agency of man exerted both purposely and unwittingly or accidentally.

Natural spread was for centuries the rule. Species dispersed under natural conditions along the line of least resistance. Winged animals and seeds were spread by flight and by the agency of winds, and at their stopping places thrived or did not thrive according as conditions were suitable or not suitable. Aquatic animals and plants and small land animals and plants were distributed by the action of rivers and streams and by the ocean itself. Wonderful migrations have occurred, commonly with birds, more rarely with other animals; ice floes and driftwood have carried animals and plants far from their original habitats and even volcanic action has taken part in the dispersal of species. Smaller animals, especially mollusks and insects, and the seeds of plants, have been carried many hundreds of miles by birds and lesser distances by mammals.

With the improvement of commercial intercourse between nations by land and by sea, another factor became more and more prominent until, in the present period of the world's history, the agency of man in the spread of species, taking all plant and animal life into consideration, has become the predominating one.

Potentially cosmopolitan forms, possibly even insular indigenes, have by this important agency become dispersed over nearly all of the civilized parts of the globe, while thousands of other species have been carried thousands of miles from their native homes, and have established themselves and flourished, often with a new vigor, in a new soil and with a novel environment.

It is obvious that this agency is readily separable into two divisions: *a*, intentional; *b*, accidental.

(*a*) *Intentional introductions.*—Since early times strange plants and animals have been carried home by travellers. Conquering armies have brought back with the spoils of conquest new and interesting creatures and useful and strange plants. With the discovery of America and with the era of circumnavigation of the globe such introductions into Europe of curious and useful species, plants in particular, increased many fold; while, with the colonization of America and other new regions by Europeans, there were many intentional return introductions of old-world species conducive to the welfare or pleasure of the colonists. Activity in this direction has been increasing and increasing. Public botanical gardens and many wealthy individuals in all quarters of the globe have hardly left a stone unturned in their efforts to introduce and acclimatize new plants, particularly those of economic importance and æsthetic quality, not failing occasionally, it must parenthetically be said, to establish some noxious weed, or some especially injurious insect; while it is safe to say that probably the majority of the desirable plants of Europe which will grow in the United States have already been introduced, and that there has been an almost corresponding degree of activity in the introduction of desirable plants from the United States into Europe. In all this host of valuable introductions there have been comparatively few which have turned out badly, aside from failures of establishment. The wild garlic (*Allium vineale*), that ubiquitous plant which gives its taste to milk, butter, and even to beef during the spring and summer months in many of our States, is said to have been intentionally introduced by the early residents of Germantown, Pennsylvania. The water hyacinth (*Piaropus crassipes*), originally grown for ornament in a pond near Palatka, Florida, escaped into the Saint John's river about 1890, and has multiplied to such an extent as seriously to retard navigation and to necessitate government investigation. The distribution of the orange hawk-weed

(*Hieracium aurantiacum*), a dangerous species which has ruined hundreds of acres of pasture land in New York of recent years, was originally aided by a florist as a hardy ornamental plant. The European woad-waxen (*Genista tinctorium*) was early introduced at Salem, Mass., in fact about thirty years after the settlement of the colony. It has apparently not been used as a dye plant, but for garden and ornamental purposes only. During the last few years it has become a noxious weed throughout Essex and the adjoining counties. Standing on a rock at Swampscott on July 9 last, the writer was able to see that the country for miles around was colored a bright yellow with enormous masses of this plant. Similar instances are fortunately rare and the majority of our noxious weeds have been accidental introductions.

Intentional introductions of animals, however, have by no means resulted as advantageously as intentional introductions of plants, with the exception of the truly domesticated species such as the horse, ass, cow, sheep, pig, dog, cat, poultry, honey bee, and silkworm of commerce. Even with such species, the grazing ranges of Australia have been overrun by wild horses to such an extent that paid hunters shoot them at a small sum per head and the European rabbit has become a much worse plague on the same island continent.

Intentional introductions of wild species, however, have almost without exception resulted disastrously.

At various intervals between 1850 and 1867 a few pairs of English sparrows were introduced into our northeastern States to destroy canker-worms, and to-day this species is a ubiquitous and unmitigated pest throughout all the austral and transition regions of North America, finding its limit only at the borders of the boreal zone, while the place of the injurious insect it was imported to destroy has been taken by another and worse insect pest which it will not touch.

In 1872 Mr. W. Bancroft Espeut imported four pairs of the Indian mongoos from Calcutta into Jamaica for the purpose of destroying the "cane-piece rat." Ten years later it was estimated that the saving to the colony through the work of this animal amounted to £100,000 annually. Then came a sudden change in the aspect of affairs. It was found that the mongoos destroyed all ground-nesting birds and that the poultry as well as the insectivorous reptiles and batrachians of the island were being extermi-

nated by it. Injurious insects increased in consequence a thousand fold, the temporary benefits of the introduction were speedily wiped away, and the mongoos became a pest. Domestic animals, including young pigs, kids, lambs, newly-dropped calves, puppies and kittens were destroyed by it, while it also ate ripe bananas, pine apples, young corn, avocado pears, sweet potatoes, cocoas, yams, pease, sugar cane, meat and salt provision and fish. Now, we are told, nature has made another effort to restore the balance. With the increase of insects due to the destruction by the mongoos of their destroyers, has come an increase of ticks which are destroying the mongoos and all Jamaicans rejoice.

The flying foxes of Australia (*Pteropus* spp.) are animals which are very destructive to fruit in their native home. Frequently some well-meaning but misguided person will arrive on a steamer at San Francisco with one or more of these creatures as pets. While it is not probable that any of the flying foxes will thrive in northern California, or in fact in Austral regions, the experiment is too dangerous a one to try, and the quarantine officer of the California State Board of Horticulture has always destroyed such assisted immigrants without mercy.

Less than thirty years ago (in 1868 or 1869), Professor Trouvelot imported the eggs of the gypsy moth (*Porthetria dispar*) into Massachusetts. The insect escaped from confinement, increased in numbers, slowly at first, more rapidly afterwards, until in 1889 it attracted more than local attention with the result that in 1890 the State began remedial work. This work has steadily progressed since that time and the State has already expended something over a half million of dollars in the effort to exterminate the insect, and it is estimated that one million five hundred and seventy-five thousand dollars more must be used before extermination can be effected.

Contrast with this a single intentional importation which has had beneficial results. The Australian ladybird (*Vedalia cardinalis*) was introduced into California in 1889 with the result of saving the whole citrus growing industry of the State from approaching extinction through the ravages of the cottony-cushion scale (*Icerya purchasi*). Later importations of the same insect into South Africa and Egypt also resulted beneficially.

We have thus had sufficient experience with intentional importations to enable us to conclude that, while they may often be beneficial in a high degree, they form a very dangerous class of

experiments and should never be undertaken without the fullest understanding of the life history and habits of the species. Even then there may be danger as, with a new environment, habits frequently change in a marked degree.

(*b*) *Accidental introductions.*— The agency of man, however, has been more potent in extending the range of species and in changing the character of the faunas and the floras of the regions which he inhabits, by means of accidental importations.

The era of accidental importations began with the beginnings of commerce and has grown with the growth of commerce. The vast extensions of international trade of recent years, every improvement in rapidity of travel and in safety of carriage of goods of all kinds have increased the opportunities of accidental introductions; until, at the present time, there is hardly a civilized country which has not, firmly established and flourishing within its territory, hundreds of species of animals and plants of a foreign origin, the time and means of introduction of many of which cannot be exactly traced, while of others even the original home cannot be ascertained, so widespread has their distribution become.

These accidental importations would at first glance seem to have been more abundant with plants than with animals, since the opportunities for the carriage of seed, especially flying or burr-like seed and especially when we consider the vitality of this form of the plant organism, are plainly manifold; but I shall later show that possibly even this obvious generalization must be modified, in view of the multitudinous chances for free travel which the smaller insects have under our modern systems of transportation.

The agencies which have mainly been instrumental in the accidental distribution of plants are:

1. *Wind storms.*— It is obvious that light flying seeds may be carried many hundreds of miles by hurricanes and may fall in new regions.

2. *Water.*— This is a very important agency in the distribution of plants upon the same continent, but less important as affecting intracontinental distribution. Still they may be carried by this means from one island to another adjoining island and when lodged in the crevices of driftwood they undoubtedly travel greater distances.

3. *Birds.*— Seeds are frequently carried great distances by birds. Many of the larger seeds will germinate after passing through the alimentary canal of a bird and may thus be eaten at

one point and voided with the excrement at a widely distant point. It has been shown, for example, that the local distribution of *Rhus toxicodendron* is greatly effected by the carriage and distribution of the seed in this way by the common crow. Smaller seeds are carried in earth on the feet of birds. Darwin's example of a wounded red-legged partridge which had adhering to its leg a ball of earth weighing 6½ ounces from which he raised 32 plants of about five distinct species is an evidence of the possibilities of this agency, while his experiment with 6¾ ounces of mud from the edge of a pond which produced 537 distinct plants, an average of a seed for every six grains of mud, is still more conclusive.

4. *Ballast.*—This is the first of the distribution methods which may be combined under the head of "agency of man." The discharge of earth ballast by vessels coming from abroad has been a notable means of distribution of plants by seed. We have just seen how many seed may germinate from a very small lump of earth and the possibilities in this direction of the many thousands of pounds of discharged ballast are very great. In fact the ballast grounds in the neighborhood of great cities are invariably favorite botanical collecting spots; they have usually a distinctive flora of their own and from these centres many introduced plants spread into the surrounding country.

5. *Impure seed.*—The great industry in the sale of seed which has grown up of late years is responsible for the spread of many plant species, principally, it must be said, undesirable species. Mr. L. H. Dewey says: "It may be safely asserted that more of our foreign weeds have come to us through impure field and garden seeds than by all other means combined."

6. *The packing material of merchandise.*—The hay or straw used in packing crockery, glassware or other fragile merchandise is a frequent carrier of foreign seeds. Such goods frequently reach the retailer without repacking, and the hay or straw is thrown out upon the fields or used as bedding for domestic animals and carried out with the manure.

7. *Nursery stock.*—Plants are often accidentally introduced by means of seeds, bulbs and root stocks attached to nursery stock, or among the pellets of earth about the roots of nursery stock. The extraordinary development of late years of commerce in nursery stock has undoubtedly been responsible for the intracontinental carriage of many species of plants in this way.

Instances of the accidental spread of larger animals by man's

agency are necessarily wanting. Of the smaller mammals the house rat and the house mouse have been accidentally carried in vessels to all parts of the world and have escaped and established themselves, the former practically everywhere except in boreal regions, or only in its southern borders, and the latter even as far north as the Pribyloff Islands, as I am informed by Dr. Merriam. Small reptiles and batrachians are often accidentally carried by commerce from one country to another, but although there are probably instances of establishment of such species none are known to me at the time of writing.

Land shells are often transported accidentally across the ocean in any one of the many ways in which the accidental transportation of plants and insects may be brought about, and by virtue of their remarkable power of lying dormant for many months are able to survive the longest journeys. The conditions which govern the establishment of species in this group, however, seem somewhat restrictive, whence it follows that comparatively few forms have become widespread through man's agency, although Binney mentions a number of European species which have been brought by commerce into the United States and have established themselves here, mainly in the vicinity of the seaport towns of the Atlantic coast.

With the earthworms, a striking situation exists. It has been shown that, "without a single exception, the Lumbricidæ from extraeuropean regions are identical with those of Europe; there is not a variety known which is characteristic of a foreign country."[1] Careful consideration of the evidence seems to show that this is due to accidental transportation by the agency of man.

Comparatively little has been done in the study of geographical distribution of insects. In the words of Wallace:

"The families and genera of insects are so immensely numerous, probably exceeding fifty-fold those of all other land animals, that for this cause alone it would be impossible to enter fully into their distribution. It is also quite unnecessary, because many of the groups are so liable to be transported by accidental causes that they afford no useful information for our subject; while others are so obscure and uninteresting that they have been very partially collected and studied, and are for this reason equally ineligible."

Nevertheless the time has already arrived with some groups, and

[1] F. E. Beddard, Text Book of Zoögeography, Cambridge, 1895, p. 153.

is not far distant even with the others which Mr. Wallace has termed "obscure and uninteresting," when, owing to the indefatigable industry of entomologists as a class, important facts can be gained along distribution lines from the group of insects. Thus, it is only within the past few months that the publication of Mr. W. F. Kirby's Catalogue of the Odonata of the World has made it possible for Mr. G. H. Carpenter, of the Royal Dublin Society, to prepare a comprehensive paper on the geographical distribution of the dragon-flies, a group in which comparatively few workers have interested themselves. It is in a measure true to-day, as it was entirely true when Wallace wrote, that "many of the groups are so liable to be transported by accidental causes that they afford no useful information for our subject;" yet, even with the group in which the greatest obscurity as to the original home of the species has existed, owing to a very easy and most frequent commercial transportation — the Coccidæ or scale-insects — the continued discovery and characterization of new forms from all parts of the world, and especially of those existing in wild regions, away from the influence of man, are gradually giving us an insight into the probable character of the original coccid faunas of more or less restricted regions.

By reason of the drawbacks mentioned, Wallace considered only "a few of the largest and most conspicuous families which have been so assiduously collected in every part of the globe and so carefully studied at home as to afford valuable materials for comparison with the vertebrate groups." These groups included the sixteen families of diurnal Lepidoptera and six of the families of Coleoptera. Even with this restriction among the beetles, however, he must have had some difficulties with the accidental importations; for, among the beetles, are hundreds of examples of this class of introductions. For instance, writing later in his Island Life, the great naturalist shows that, in 1880, the total number of species of beetles known in the Azores amounted to two hundred and twelve of which one hundred and seventy-five were European. Out of these, however, no less than one hundred and one were believed to have been introduced by human agency. Concerning St. Helena he quotes Mr. Wollaston's opinion that seventy-four of the two hundred and three species have certainly been introduced by the agency of man.

In considering the question as to the regions with which an

interchange of forms is most likely to occur, it is obvious that they are those which have the greatest similarity of climate and, most nearly, identity in point of time of seasons, those in fact which are most likely to afford similar environmental conditions. A study of the similarity of faunas and floras already existing will lead us to the same result. Wallace has pointed out that with the Coleoptera the best marked affinities between regions are those between the Nearctic and the Palæarctic, the Oriental and Australian, the Australian and the Neotropical, all of which appear to be about equal in each case. Next comes that between the Ethiopian and the Oriental on the one side and the Ethiopian and the Neotropical on the other, which also appear about equal. Then follows that between the Nearctic and Neotropical regions and lastly, and by far the least marked, that between the north temperate and south temperate regions.

Further, in the consideration of accidental commercial importations, the amount and frequency of commercial interchange and the rapidity of the journey are most important factors.

From all of these considerations combined we arrive at the conclusion that the regions with which accidental interchange of species should be most frequent are Europe and North America, and this is with insects to a certain extent borne out by the facts. The introduction of species from Europe into North America is of every day occurrence and their establishment is far from rare. The carriage of American species to Europe is an equally frequent matter but their establishment is much less frequent.

I have studied in this connection, my profession being that of an economic entomologist, principally the species which are prominent as injurious to horticulture or agriculture or in other ways inimical to man. Listing the insects of prime economic importance to the United States, the species each of which almost annually causes a loss of hundreds of thousands of dollars, we find that they number seventy-three. Of these thirty are native, while thirty-seven species have been introduced, six species being of doubtful origin. Of the thirty-seven introduced species, thirty have come to us from Europe, all, with one exception, as accidental importations.

Of the prominent European injurious insects, on the other hand, but three are said to have come from America: the grape-vine Phylloxera (*Phylloxera vastatrix*), the woolly root-louse of the apple or "American blight" (*Schizoneura lanigera*), and the Medi-

terranean flour moth (*Ephestia kuhniella*). Of these but one is certainly American — the Phylloxera. The origin of the Schizoneura is somewhat doubtful, while the Mediterranean flour moth is not American, but probably came to us from Europe although originally it is probably Oriental.

As with these insects of prime economic importance, so it is with other less noted species. There have been rather frequent establishments of European species in America, but practically none of American species in Europe. The reason for this curious condition of affairs is difficult to find. The general trend of accidental importations seems to have been westward and it is doubtless a fact that certain of our now cosmopolitan forms were originally Asiatic and have travelled westward through Europe, to and across America, and thence to Hawaii, New Zealand and Australia. The existence of such a law is borne out in the study of plants as well. The statement just made regarding insects of prime economic importance is almost exactly paralleled with the plants classed as weeds. It has thus been shown that out of two hundred American weeds, one hundred and eight are introduced, of which ninety-six are from Palæarctic regions, sixty-eight being native to Europe, while it seems that less than half a dozen American species have become troublesome in Europe. A number of American species, however, have been carried to Australia and flourish there with vigor.

This general trend from east to west has always been in the direction of the newer civilization — from the older civilization to the newer. That this in itself is significant cannot be doubted and, in the case of the insect and plant enemies of agriculture, the facts surrounding this condition are almost in themselves sufficient to account for this directive movement. I have shown in another paper that the denser population of the older countries and the resulting vastly smaller holdings in farms, the necessarily greatly diversified crops, the frequent rotations of crops, together with the clean and close cultivation necessitated by the small size of the holdings, and the cheaper and more abundant labor, will all operate as a barrier against the establishment of injurious species; while the reversed conditions, in a newer country, at once liberate an introduced species from the repressive conditions which affected it in its original home and encourage its establishment, multiplication and spread. But there are deeper causes than this at work. It has been suggested that the flora and fauna of America, the

older continent, have become degenerate through age and cannot successfully resist competition with the more vigorous forms introduced from the younger continent of Europe[1] and that there are not-yet-formulated climatic differences favorable to the development of Palæarctic forms on the American continent; but these theories seem unsusceptible of proof and for the present we must content ourselves to accept the facts as we find them.

The insects which are accidentally imported are carried in three main ways: either (1) they are unnoticed or ignored passengers on or in their natural food, which is itself a subject of importation, such as nursery stock, plants, fresh or dried fruit, dried food stuffs, cloths, lumber, or domestic animals; or (2) their food being the packing substances used to surround merchandise, or the wood from which cases are made, they are thus brought over; or (3) they may be still more accidental passengers, having entered a vessel being loaded during the summer season, and hidden themselves away in some crevice. The coleopterists (Hamilton and Fauvel) make a distinction by name among these classes, calling the first group "insects of commerce" and the last "accidental importations."

It would appear on the face that these more strictly accidental importations must be rarer than those which are termed commercial importations, yet of the 156 introduced Coleoptera recorded by Dr. Hamilton in 1889, sixty only were considered by that writer as insects of commerce, while ninety-six he thought had been brought over in this accidental way.

The remaining Coleoptera common to North America and the Palæarctic region, 278 in all, Dr. Hamilton considers to be practically circumpolar species, or at least not imported. Fauvel, in his remarks and additions to Hamilton's catalogue, raises the number of non-introduced or circumpolar species to 366, leaving 125 as imported, of which only twenty-eight appear to have been imported from temperate Europe, the rest being cosmopolites or subcosmopolites. Of the latter class he thinks that fifty-nine originally came from the temperate Europeo-Siberian fauna, ten from the Oriental fauna, fifteen from the Ethiopian, four from the Neotropical, seven being uncertain and two unknown.[2]

[1] The spread of European species in Australia has been explained by the superior energy of the younger races of the Palæarctic region.

[2] Hamilton in his 1894 paper raises the number of Coleoptera common to the two continents to 594, making the number of introduced species, 216.

It should be noted, however, that there is grave room for difference of opinion regarding a number of the European species considered by him as indigenous to North America. *Scolytus rugulosus*, *Hylastinus trifolii*, *Anthrenus scrophulariæ*, *Sitones hispidulus*, *Cryptorhynchus lapathi*, and a number of others which might be specified, for example, have undoubtedly been imported from Europe and were quite possibly originally imported.

There are obstacles in the way of the establishment and spread of species which are imported quite by accident, which usually do not exist in the case of the so-called commercial importations. In many cases, entering the vessel by accident, they exist there as single individuals and upon liberation, even should the conditions be favorable, only gravid females could perpetuate the species. Then also the majority of such specimens are liberated, upon the unloading of the vessel, upon the wharves. The water front of a seaport city is not a favorable place for the establishment of a species which feeds on living vegetation. Frequently, even when it is a species well fitted for acclimatization, it will have to fly or be carried for miles inland before it can find a place possible for the establishment of the species. So it happens that while foreign insects are frequently found in living condition about the wharves of our larger seaports during the summer months, almost none have succeeded in getting a foothold in the vicinity. Mr. Otto Lugger, when living in Baltimore, Md., made a collection of many species of foreign insects found upon the wharves, yet he has recorded the establishment of but a single species, viz., *Aphodius erraticus*, a European dung-beetle, which managed to get to Druid Hill Park where it bred in the dung of the tame deer, afterwards spreading into the surrounding country and breeding in the dung of sheep and other domestic animals.

Practically, therefore, after many years of the most active commerce, the insect faunas of the immediate vicinity of the larger seaports like New York, Boston, Philadelphia and Baltimore, have not been greatly changed by the introduction of foreign elements.

All of the household insects and true city insects are of course exceptions to this conclusion and the strong flying, vigorous, and simple living dipterous insects — the very ones most likely to enter a loading vessel and to escape on the discharge of its cargo — will many of them find proper places for breeding. It is likely that a much larger proportion of the many species of Diptera common

to Europe and North America have been brought over in this accidental manner than is the case with the Coleoptera.

But often these purely accidental species are carried inland in packing cases into the cracks of which they may have crawled, or even in the trunks of passengers, and they may then be liberated in more favorable localities. For example, Mrs. H. G. Hubbard, after spending the summer on Prince Edwards Island, returned in the autumn to Detroit, Mich., and in unpacking her trunks her husband found two specimens of *Phytonomus punctatus*, a species not previously known to occur in Michigan, although found there in injurious numbers a year or so later. It is altogether likely that the imported elm-leaf beetle (*Galerucella luteola*), an insect which enters houses for hibernating purposes, was first brought to America in this manner.

It is, however, the accidental commercial importations which theoretically stand the best chance of establishing themselves, since, in the first place, they are generally imported in or upon their natural food. In the second place, they generally occur in considerable numbers instead of as isolated individuals as with the more purely accidental importations; and, in the third place, they are usually carried as originally packed, far from the port of entry.

With insects brought over on plants or nursery stock the conditions could not well be much more favorable. Their supply of food is looked after with care, the host plant is soon put in the ground under the best of surroundings, and the greatest care is taken of the choice importation. Upon or in importations of this kind are carried Coccidæ in all stages of growth (and often, fortunately, their enclosed parasites), the eggs of Aphididæ, the larvæ of wood-boring Coleoptera, the eggs of many other insects, the cocoons of small Lepidoptera, and probably even in rare cases the larvæ of Lepidoptera, since it now seems likely that *Euproctis chrysorrhœa* was imported into Massachusetts on nursery stock in its larval hibernacula. The Coccidæ, however, are most abundantly carried in this way. Under natural conditions these insects have usually a rather restricted distribution, but by means of this commercial distribution many of them have become of almost world-wide range, and the end will certainly not be reached until every country possesses every species of scale insect which can possibly live in its climate. A few instances drawn from a recent paper by Mr. Cockerell will illustrate this fact:

Diaspis amygdali, or *lanatus*, was described from Australia in 1889. To-day we know it from Australia, Ceylon, Hong Kong, Japan, several localities in the United States, Jamaica, San Domingo, Grand Cayman, Barbadoes, Martinique, Trinidad and Cape Colony.

Aulacaspis rosæ was described from Europe, but is now found also in the United States, Australia, New Zealand, the Sandwich Islands, China and Jamaica.

Chionaspis citri was described from Louisiana and Cuba in 1883 and is now known from Trinidad, Antigua, Demarara, Bermuda, Mexico, Tonga, New Zealand and Australia.

Howardia biclavis was described in 1883 from specimens on hot-house plants in Washington, D. C. Now it is known out-of-doors from Trinidad, Mexico, Tahiti, Sandwich Islands and Ceylon.

Lecanium oleæ is found in Europe, the United States, the West Indies, Mexico, Sandwich Islands, New Zealand, Australia and Cape Colony.

The hymenopterous parasites of the Coccidæ, by virtue of their mode of life, have spread almost equally with their hosts by means of this commercial transportation. I have been able to show recently that by this means a number of species of the Chalcidid subfamilies Aphelininæ and Encyrtinæ have in comparatively recent years become cosmopolites. For example:

Aspidiotiphagus citrinus, originally described from California in 1891, is now found in many other portions of the United States, in the West Indies, Italy, Austria, Ceylon, China, Formosa, Japan, Cape Colony, Queensland, South Australia and Hawaii. Practically the same remarkable distribution is followed by *Prospalta aurantii*, *Aphelinus mytilaspidis*, *A. diaspidis* and *A. fuscipennis;* while the remarkable and system-breaking Encyrtine, *Arrhenophagus chionaspidis*, described by Aurivillius from Swedish specimens in 1888, has since been found in Austria, Italy, several portions of the United States, Ceylon, Japan, Formosa and China.

Only second to the Coccidæ in the facility with which they are transported in this way are the Aphididæ. These insects, however, are fragile, soft-bodied and unprotected. They are really carried, however, in the winter-egg condition and many species are rapidly becoming cosmopolitan. They have not been studied elsewhere than in Europe and the United States and the extent to which this commercial distribution has been carried can only be surmised. A

suggestion of this extent occurred to me when, within the past few weeks, specimens of *Aphelinus mali*, a common parasite of Aphididæ in Europe and North America, were received from such a comparatively out-of-the-way corner of the world as Passaroean, Java.

Other still smaller and still less studied insects are undoubtedly carried by this method of transportation, as the recently discovered identity of certain North American Thysanoptera with those of Sweden and Russia would seem to show. The small plant-feeding mites of the family Phytoptidæ are particularly subject to this form of commercial distribution and when they are fully studied it will doubtless be found that many forms have become subcosmopolitan.

Of larger insects, nearly all of the wood-boring beetles common to Europe and the United States have probably been brought over in this way. *Zeuzera pyrina*, the large wood-boring cossid moth, also probably came over on living plants and, as I have just stated, the newly imported brown-tail moth, *Euproctis chrysorrhœa*, was probably imported at Boston with nursery stock.

Careful observations on the insects transported with this class of merchandise have never been made, except possibly at the port of San Francisco. At this port the State Board of Horticulture has established under State laws a quarantine for all incoming plants and fruit. The entomologist and quarantine officer, Mr. Alexander Craw, has entire jurisdiction over such articles consigned to points within the State, and examines, destroys and fumigates, at his discretion. He has not, however, in his reports, given us complete lists of the insects collected in this work, although I understand from persons who have visited his office that he has preserved collections of the more important species from an economic standpoint. The vessels examined have, almost without exception, come from Pacific ports and the difficulty of naming insect material thus received would be very great. It is this fact which has probably hitherto prevented the publication of a general list. A list of the scale insects, however, has been published.[1]

Between July 2, 1894, and August 29, 1896, Mr. Craw inspected two hundred and thirty-two vessels carrying plants or other articles liable to be infested with living insects, and consigned to California individuals or firms. One hundred and twenty-two lots he found clean and passed; forty lots he admitted after fumigation;

[1] Bull. 4. Tech. Ser. Div. Entom. U. S. Dept. Agric. 1893, pp. 40–41.

twenty lots he destroyed, and seventy-eight lots he destroyed in part. One lot, consisting of one thousand boxes of apples, he sent back on the refusal of the owner to allow them to be fumigated.

Living plants and nursery stock afford then, perhaps, the most certain means for the accidental transmission and subsequent establishment of many kinds of insects. Commerce in objects of this class is rapidly increasing and has already assumed considerable proportions, the imports into the United States alone in the fiscal year, ending June 30, 1896, having reached a value of nearly $1,000,000, while the previous year they amounted to something over $600,000.

No great elaboration will be needed concerning the importation of foreign insects upon fruits, fresh and dry, other dry food-stuffs, cloths, lumber, or domestic animals. Fruits were imported into the United States in the fiscal year 1895–6 to the value of nearly $20,000,000, and unquestionably upon imported fruits are carried many insects. The opportunities for the establishment of species coming with fresh fruits, however, are obviously slight as compared with those which come on living plants or in dried food-stuffs; and, as a matter of fact, it appears that already nearly all of the dried food-insects have become cosmopolitan. The same may be said of the insects which affect domestic animals. The forms which are truly parasitic in the larval stage have most of them been carried everywhere, while the other forms, which attack domestic animals only as adults, have some of them been carried far and wide. As an example we may recall the European *Hœmatobia serrata* which was brought to New Jersey probably in 1886, and which has spread over the entire country from Maine to California.

The fact that insects may be and doubtless are transmitted in the material used in packing heavy or delicate merchandise must not be overlooked. We have already shown that dangerous weeds have been transmitted in this way, and when the material is hay or straw the danger of importing certain injurious insects becomes great. *Cecidomyia destructor*, the well-known Hessian fly, is supposed to have been first brought to the United States from Europe in straw bedding on troop vessels during the War of the Revolution, and to have recently been carried from Europe to New Zealand in the straw packing of merchandise. Laws recently proposed in New Zealand, Australia and Cape Colony, provide that such

straw or hay packing shall be burned immediately the case is opened. Dr. H. Loew, in his well-known paper, Ueber die Diptern-fauna des Bernsteins, has shown that several of the species of the genera Oscinis and Chlorops have gained a wide distribution through commerce, and this probably happened through their occurrence in hay or straw used as packing since they live in the stems of grains and grasses. Dr. Loew, by the way, considered the Diptera, by virtue of the simple conditions required for their existence, to be peculiarly susceptible to commercial and accidental distribution and was inclined to believe that the majority of the many species common to Europe and North America have been imported into the latter country. He understood, however, the existence, of a circumpolar fauna and wrote wisely and learnedly about the common ancestry of what he called analogous species. Whatever may be the cause, the Diptera seem fitted in the individual to withstand widely differing environmental conditions. The group as a whole has apparently little faunistic value either along broad lines or in a more restricted way. There are comparatively fewer characteristic genera in the main faunal regions of the world than in other groups of animals, and in our own country there are comparatively few species of restricted distribution. Very many individual species range through the Lower and Upper Austral, through the Transition and into the Boreal regions.

Aside from the Diptera, grains and grasses all over the world are subject to the attacks of a host of insects of all kinds, many of which hibernate on or within the stems, so that the proposed legal provisions of the English Colonies mentioned are by no means unwise. The substitution of the wood material known as "excelsior," the use of which is becoming so common in this country, or of some other packing material, will shortly do away with a large share of this danger.

There are, of course, other less important methods by which insects may be transported, such as in earth or damp moss about the roots of plants and in sand used for ballast. These methods, however, are not very important as a rule, although it is stated that the destructive chigoe (*Sarcopsylla penetrans*) was carried in ballast in 1872 on a vessel from Rio Janeiro to the coast of Guinea, where it has established itself most perfectly, having been found two hundred miles inland by Stanley.[1]

[1] Die Umschau, July 17, 1897, p. 528.

There remains one more source of accidental introductions and it is one which has been reasonably prolific as regards insects on several occasions. I refer to international expositions which are now becoming of almost annual occurrence. At the Centennial Exposition at Philadelphia in 1876 the insects occurring in the exhibits, especially of foreign grains, received some study by Dr. Riley who published a short note in the Proceedings of the St. Louis Academy of Science for October 2, 1876. A special committee of the Philadelphia Academy, consisting of Drs. Horn, Leidy and Le Conte also prepared and published a report at this time, but none but well known and cosmopolitan forms were found. I am not familiar with the results of any studies of a similar nature made at the Paris Exposition Universelle of 1889, but have seen the title of a paper by M. Decaux which reads "Etudes sur les insectes nuisibles receuillis à l'Exposition Universelle," Paris, 1890, which however I have not been able to consult.

In 1893, careful observations were made at the World's Fair at Chicago by Mr. F. H. Chittenden, the results of which were published by Dr. Riley in Volume VI of Insect Life. Insects to the number of one hundred and one species were found in grain and other stored vegetable products. Seven species were found affecting animal products and thirteen wood-feeding species were found in the forestry building. The interesting and significant fact is mentioned in this article that there was an exchange of seed samples between the representatives of different countries which would of course greatly facilitate the spread of seed-inhabiting insects, and it was further shown that thousands of samples were taken away from open bags by visitors from all parts of this country and probably from other parts of the world. Moreover, at the close of the Exposition the sheaves of cereals used in the decorations were taken away by armsful by visitors. After summarizing the habits and countries of origin of the different species, however, Dr. Riley expressed the opinion that no dangerous importations were made at this time. It seems altogether likely, however, that *Phyllotreta armoraciæ*, a European species which has established itself in northern Illinois, Iowa and Wisconsin since 1893, and which was found by Mr. Chittenden in that year in vacant lots near the Exposition Grounds, was an Exposition importation. Moreover an interesting Calandrid of the genus Tranes, the species of which are all Australian, has established itself injuriously in green-

houses in St. Louis as the result of the introduction of two plants of *Zamia spiralis* which were bought at the World's Fair. With these instances in mind we cannot but admit that other species heretofore overlooked probably escaped and have become acclimatized as the result of this Exposition, and that such occasions, occurring as they do more and more frequently and drawing constantly increasing material from all parts of the world will, unless precautionary measures are instituted, afford more and more frequent opportunities of a very favorable kind for the spread of injurious species.[1]

We have thus seen how great the opportunities are under our modern conditions for the transportation, in proper condition for establishment, of insects of many groups; and from this point of view it seems strange, in view of the very numerous importations, that more species do not become acclimatized even in North America where perhaps we reach the greatest possibilities in this direction. Our most intimate commercial relations are with the great faunal region most like our own, and these relations are rapidly growing both with Europe on the east and with Asia on the west, although our Asiatic importations are more abundant from the Oriental region than the Palæarctic, and from the Oriental we are not so likely to receive species which will acclimatize themselves. We have already pointed out that the faunistic relations with the Coleoptera (and undoubtedly with other groups) are least marked between the north temperate and south temperate regions and this distinction is never likely to be disturbed by imported species on account of the diametrically opposite seasons. A species starting from Argentina in the height of summer will reach the United States in the dead of winter at a time least likely to favor its acclimatization. This point was first suggested by my colleague, Mr. E. A. Schwarz, in his paper entitled "The Coleoptera common to North America and other Countries" (Proc. Entom. Soc. Wash. I, 182–194).

It appears from what we have shown that very many species are constantly being imported which do not become acclimatized. Many of the European species which we should most expect to

[1] During the later months of the Chicago World's Fair precautionary measures were instituted under Mr. Chittenden's supervision. Much dry food material was fumigated with bisulphide of carbon, and many samples which were very badly infested, were burned. At least four new and dangerous species of insects were destroyed in this way.

take hold in this country have not done so, while with others it is the unexpected which has happened. As Osten Sacken says, speaking of the Diptera, "Importation will not occur for centuries in cases where it might be expected from day to day; and again, it will sometimes take place under circumstances most improbable, and, *a priori*, impossible to foresee" (Proc. Entom. Soc. Lond., 1884, p. 489). Why should the well-known *Pieris rapæ* have made its appearance in this country and spread far and wide, while the equally common and injurious *Pieris brassicæ* and *P. napi* have never been found here? Why should *Phytonomus punctatus* have flourished with us when it is hardly known as a clover enemy in Europe, and when the congeneric *Phytonomus meles* of Europe has never been found here? Why should *Coleophora laricella* have established itself here and none of the other European Coleophoras (some of them of much greater distribution, and hibernating in cases of protective coloration and shape and attached to plants) have acclimatized themselves amongst us? Why should *Calliphora erythrocephala*, *Cyrtoneura stabulans* and *Stomoxys calcitrans* have been brought over at an early date and flourished to excess in America and many other countries, while *Sarcophaga carnaria* is unknown in any of them?

Mr. Schwarz has phrased it: "We stand here before some great unknown factor, viz., the individual character and inmost nature of the species which governs the introduction or non-introduction of each species — a factor which is variable according to each species . . ." But there is no reason why a mystery need be made of this condition. In a word, it is the capacity of the individual species to accommodate itself to a more or less novel environment. Nowhere in the whole animal kingdom do we find the natural environment more complicated than with insects. Conditions are frequently dependent upon conditions in an almost endless chain. The phenomena of fatal parasitism are of vital importance as determining the abundance of the species and are curiously complicated. I have recently proven the existence of several fatal tertiary parasites and the probable existence of quaternary parasites with *Orgyia leucostigma* in Washington. Upon the condition of this chain of interdependencies rests the welfare of the primary host. If adverse conditions affect the quaternary parasite, the primary host suffers, for the tertiary parasites increase and kill off the secondary parasites, allowing an increase of the primary para-

sites which kill off the Orgyia. The famous instance of Darwin, in which he showed that in a measure cats are responsible for the production of clover seed in England through the interrelations of cats, field mice and bumble-bees, is paralleled and outdone again and again among insects. Further, in no group of animals are the characteristics termed special protective resemblance and special aggressive resemblance, to say nothing of protective and aggressive mimicry, so well marked and so important in the life of the species as with the insects. *It is upon the degree of simplicity of its life — the degree of simplicity of its normal environment as a whole — that the capacity of a species for transportation and acclimatization, even into a parallel life zone, depends.*

Nevertheless I am fully convinced that very many more species will stand transportation from the Palæarctic to the Nearctic, from the Australian to the Oriental and the Neotropical regions than would be supposed from a consideration of these points and from a knowledge of the comparatively few forms which have as yet been transported and acclimatized. Aside from the forms brought in with their food and thus under the most favorable conditions for establishment, it is only by a lucky chance, with the average accidental insect immigrant, that it finds conditions for reproduction — a chance which may not occur once in very many times. Osten Sacken has pointed out that *Eristalis tenax* must have been brought here many times during four hundred years before it succeeded in establishing itself. Undoubtedly many of these immigrants die upon our wharves when a lucky chance, like crawling upon the clothes of a person and thus being carried out into the country, might have resulted in the establishment of the species. Given the *most favorable conditions* and many species will be able not only to accommodate themselves to a new environment, but certain of them will thrive better in the new than in the old. The effort to transport beneficial species from the Australian region and acclimatize them in the Nearctic region seemed a rash and unprofitable experiment on its face, and I confess that I for one had little hope of its success, yet it was successful with several species, and transcendently successful with one.

Much has been written of late about the success of the work in the introduction of beneficial insects by Mr. Albert Koebele into Hawaii under the auspices of the Hawaiian Government. Some of the introductions seem without doubt to have been strikingly

successful. Mr. R. E. C. Perkins has reported at some length upon this success,[1] and in commenting upon its reasons says:—

"It becomes natural to ask why the success of the imported beneficial insects has been so pronounced here, while in other countries it has been attained in a comparatively small measure. The reason, I think, is sufficiently obvious. The same causes which have led to the rapid spread and excessive multiplication of injurious introductions, have operated equally on the beneficial ones that prey upon them. The remote position of the islands, and the consequently limited fauna, giving free scope for increase to new arrivals, the general absence of creatures injurious to the introduced beneficial species, and the equability of the climate, allowing of almost continual breeding, may well afford results which could hardly be attained elsewhere on the globe The keen struggle for existence of continental lands is comparatively non-existent, and, so far as it exists, is rather brought about by the introduced fauna than by the native one."

Mr. Perkins' reasons are all good, but he has not mentioned one prime reason of success, and that is that the most successful of the imported species have come from another portion of the same great faunal region; while others have been received from the region most closely allied, viz., the Oriental.

Wallace took the view that the effectual migration of insects is perhaps, more than with any other class of animals, limited by organic and physical conditions. "The vegetation," he says, "the soil, the temperature, and the supply of moisture, must all be suited to their habits and economy; while they require an immunity from enemies of various kinds, which immigrants to a new country seldom obtain."

There is much truth in this statement, but it must be remarked that, in practical experience, immunity from enemies of various kinds is what insect immigrants find, not what they leave behind them. It takes some time before they weave into a new chain of organism preying upon organism. Our insect importations from abroad when they are of economic importance, and those from Europe, are very likely to be of such importance, leave their old insect enemies behind them and frequently are not readily attacked by native ones. These last accommodate themselves to the new comer in time, but for a while he enjoys comparative immunity.

[1] Nature, Vol. 55, No. 1430, March 25, 1897.

The rapid multiplication and spread of *Pieris rapæ*, of *Hæmatobia serrata*, of *Phytonomus punctatus*, of *Porthetria dispar*, of *Anthonomus grandis*, of *Icerya purchasi*, and many others may probably be principally laid to this cause.

I should be remiss did I not refer to another aspect of the accidental introduction of species, viz., that it not only adds species to a native fauna, but also that it often causes the disappearance of native forms. Since the establishment within our boundaries of *Pieris rapæ*, our native *Pontia oleracea* has almost entirely disappeared in localities in which it formerly abounded and in some sections has entirely disappeared. Since *Doryphora 10-lineata* came east and multiplied upon the cultivated potato in such prodigious numbers, the formerly common eastern *Doryphora juncta* has become a rare species. Walsh pointed out thirty years ago that one effect of the westward spread of the European *Mytilaspis pomorum* was to cause the gradual local disappearance of the native *Chionaspis furfurus*. Hubbard has shown that the increase of the imported *Mytilaspis citricola* in Florida was followed by the decrease of *Mytilaspis gloverii*, which, though not native, was an earlier importation, — a most interesting and, so far as records go, unique case. Instances might be multiplied which will show that the establishment of foreign species thus often produces at least a dual effect on the character of the fauna as a whole.

In closing it will not be inappropriate to point out that the accidental importation of species is only one of the ways in which the agency of man is altering the character of native faunas, and that in spite of its extent it is really the least of the ways. The influence of civilization is immediately destructive to natural floras and faunas. It is already too late to gain an adequate idea of natural conditions in even recently settled portions of the globe. Wallace has dwelt upon the comparatively scanty and unimportant results to Natural History of most of the great scientific voyages of the various civilized governments during the present century from which it has resulted "that the productions of some of the most frequently visited and most interesting islands on the globe are still very imperfectly known, while their native plants and animals are being yearly exterminated. . . . Such are the Sandwich islands, Tahiti, the Marquesas, the Philippine Islands, and a host of smaller ones; while Bourbon and Mauritius, St. Helena and several others have only been adequately explored after an

important portion of their productions has been destroyed by cultivation or the reckless introduction of goats and pigs" (Island Life, p. 7).

Elsewhere he shows that the introduction of goats into St. Helena utterly destroyed a whole flora of forest trees; and with them all the insects, mollusca and perhaps birds dependent upon them. And further, that "cattle will, in many districts, wholly prevent the growth of trees; and with the trees the numerous insects dependent on those trees, and the birds which feed upon the insects, must disappear as well as the small mammalia which feed on the fruits, seeds, leaves or roots." Many local American instances have been brought together, by Mr. F. M. Webster, in an important paper entitled "Biological effects of civilization on the insect fauna of Ohio," which comes to me as I write these closing lines.

But the purpose of this address has been to dwell solely upon the question of the spread of species and I must not touch upon other topics however closely akin.

It seems to me that the practical point to which we must come, after summarizing all that has been shown, is that since so many species have been imported by pure accident and have acclimatized themselves, may not much be accomplished by wisely planned and carefully guarded introductions. The somewhat haphazard, but none the less important and skillful work of Albert Koebele, first for the United States Government, and now for the Hawaiian Government, is certainly an indication, taken in connection with what we have shown, that thorough experimental work with predaceous and parasitic insects promises, in especial cases, results of possibly very great value.

We wish no more destructive birds like the English sparrow: we have no desire to make an American resident of the Indian mungoos, nor have we any desire to import the Australian flying fox as a pet. Still less do we desire to allow any more European plants to escape from cultivation and emulate the Russian thistle. But there are many absolutely beneficial insects of Palæarctic regions which might flourish amongst us and whose intentional introduction could not be harmful from any point of view, while they might be of the greatest service.

PAPERS READ.

[ABSTRACTS.]

On the relationships of the Nematognaths. By Prof. Theodore Gill, Columbian University, Washington, D. C.

The Nematognaths have been considered by most ichthyologists to be most nearly related to the Ganoids or even associated with them in the same order. Their entire structure, however, indicates that they are most nearly related to the Plectospondyls and they may be associated even in the same superorder for which the name Ostariophysi of Sagenmeh may be adopted.

On a collection of Cephalopoda from the Albatross expedition. By Wm. E. Hoyle, Owens College, Manchester, England.

On a Sarcostyles of the Plumularidæ. By Prof. C. C. Nutting, State University of Iowa, Iowa City, Ia.

The morphology of the sarcostyles has been investigated by various writers from the middle of the century to the present. They are composed of ectoderm, endoderm and, according to most writers, a solid axial rod.

A body cavity was described by one of the earliest writers and re-described by the present writer. The sarcostyles are undoubtedly degraded "persons" of the hydroid colony.

The function of these structures is not well understood. It appears to be partly defensive, partly prehensile and partly offensive. The function of scavengers and digestive organs has also been ascribed to them.

NOTES ON THE DEVELOPMENT OF DRASTERIA ERECHTEA. By Prof. F. M. WEBSTER, Wooster, Ohio.

THE paper gives the results obtained from rearing larvæ from eggs to adults, and shows the individual variation of larvæ and moths both in habit and appearance, individual variation being the object of the studies.

BROOD XVI OF CICADA SEPTENDECIM IN OHIO. By Prof. F. M. WEBSTER, Wooster, Ohio.

THE paper deals with the occurrence of Brood XVI of *Cicada septendecim* in Ohio, in 1897. The area over which it has been observed is indicated; the influence of natural enemies is discussed and the probabilities of its becoming extinguished explained.

NOTES ON THE EMBRYOLOGY OF THE PIG. By Dr. CHARLES SEDGWICK MINOT, Harvard Medical School, Boston, Mass.

(1) *The hypophysis*. There is a true infundibular gland, which is homologous with the *Saccus vasculosus* of teleosts, and is identical in form and connections with the embryonic saccus. The duct of the gland becomes a solid stalk.

There is also a true hypophysis, which develops a vestibule and gland-tubes homologous with the same parts throughout the vertebrate series.

(2) *The cardinal vein*. It is a striking peculiarity of transverse sections of pig-embryos that in the region of the Wolffian body no distinct vein appears, corresponding to the cardinal vein, usually found on the dorsal side of the Wolffian body. This is due to the fact that in the pig the cardinal vein, after entering the cephalic end of the Wolffian body, breaks into a multifid sinus, in which are lodged the Wolffian tubules. Another peculiarity of the cardinal vein is that it does not join the jugular to form the transverse *Ductus Cuvieri*, but joins instead the great hepatic vein (*Ductus arantii*) not far from the heart.

(3) *Allantoic villi*. The stalk of the Allantois begins to form on its cephalad side within the abdomen a series of lobulated outgrowths which gradually increase until they occupy a relatively considerable space in the abdomen. These villi consist of a cuboidal mesothelium covering a mass of matricial or ground substance, in which are scattered a few cells of mesenchymal character. The villi are beginning their development in embryos of 14.0–17.0 mm., and are already reduced in embryos of 24.0 mm. The author has no surmise to offer as to their function. Nothing similar to them is known in other mammals.

HARVARD EMBRYOLOGICAL COLLECTION. By DR. CHARLES SEDGWICK MINOT, Harvard Medical School, Boston, Mass.

THE collection consists mainly of series of sections of vertebrate embryos. It is expected ultimately to have representatives of several divisions of the Mammalia, and of the principal types of non-Mammalian vertebrates.

The work of forming the collection is going forward with following forms:

Man	Necturus
Dog	Amia
Pig	Amiurus
Rabbit	Batrachus
Chick	Acanthias
Lacerta	Torpedo
Frog	Petromyzon
Amblystoma	Amphioxus

The plan is to select for each form a carefully graduated series of stages, and to make of each stage three complete series of sections in three planes, the transverse, the sagittal and the frontal, or if the embryo is curved or twisted the three planes are chosen at right angles to one another. A drawing of each stage is made. A double catalogue is kept; the first in book form is the entry-catalogue, and recalls all details of preservation, cutting, staining and mounting. The series of sections are numbered according to their order of entry. The second catalogue is on cards, which are arranged first according to species, second under each species according to stage. Every series of sections has a separate entry number, and a separate card. Every section is counted and the sections of every series numbered. At present time there are about one hundred series completed.

The collection is intended primarily for investigators, and is open to all competent investigators working in the Embryological Laboratory. It is hoped that it will increase, during years to come, in size and still more in usefulness. So far as known to the writer, it is the first collection of the kind to be started.

CHARACTERS FOR DISTINGUISHING THE NORTH AMERICAN SPECIES OF CERESA. By WM. H. ASHMEAD, U. S. National Museum, Washington, D. C.

AN explanation of the application of recently discovered morphological characters to the classification of the homopterous genus Ceresa. The author points out the fact that many of the genera in the Membracidæ are merely dimorphic forms of other genera.

RECONSTRUCTION OF PHENACODUS PRIMÆVUS, THE MOST PRIMITIVE UNGULATE. By Prof. HENRY F. OSBORN, Columbia University, New York, N. Y.

THIS paper was accompanied by a photograph of the remounted skeleton of Phenacodus and a wax model executed by Charles Knight. As originally mounted in Professor Cope's laboratory the famous skeleton of *Phenacodus primævus* conveyed a very imperfect impression of its actual form and proportions. Several serious errors were committed by the restorer, the most important of which was the implanting of two of the cervical vertebræ in the tail. The author, therefore, considered it advisable to completely remount the animal, and this has been done by Mr. Adam Hermann and Mr. Martin of the American Museum, at an expenditure of five months' time.

The animal is placed as nearly as possible in a natural position. It shows that the feet were not plantigrade, or soled upon the ground, but digitigrade as in the tapir. The body is characterized by the great convexity of the back, characteristic of the carnivore and of all early ungulates. A further carnivorous feature is the great development of the hind quarters and of the tail. The disproportion between the hind and the forequarters is heightened by the extremely small size of the head, containing a brain which was about the size of that of the opossum, as fully described by Cope.

HOMOLOGIES AND NOMENCLATURE OF THE ELEMENTS OF THE MOLAR TEETH. By Prof. HENRY F. OSBORN, Columbia University, New York, N. Y.

AFTER a brief review of the tritubercular theory of Cope, the writer spoke of his hypothesis advanced at the American Association meeting in 1891 that the multitubercular teeth of the Multituberculata and Monotremata were also of tritubercular origin. This hypothesis seems now to be confirmed by the teeth of Gomphodontia, especially of the genus Diademodon, by Professor Seeley in the Karoo Beds of South Africa, of Permian age.

The various Gomphodonts in these beds present molar teeth of more or less regular tritubercular pattern. Related to them in skull structure is Tritylodon, a typical multituberculate. This indicates that from the trituberculate Gomphodonts Tritylodon and other multituberculates may have taken their origin. The paper was discussed by Professor Minot and Dr. Gill.

MODIFICATION AND VARIATION AND THE LIMITS OF ORGANIC SELECTION. By Prof. HENRY F. OSBORN, Columbia University, New York and Prof. EDWARD B. POULTON, Oxford University, England.

(Abstract of Professor OSBORN'S paper.)

ORGANIC selection is the term proposed by Prof. Mark Baldwin and adopted by Prof. C. Lloyd Morgan and the author, for a process in nature which is believed to be a true cause of definite or determinate evolution in certain stuctures. The hypothesis is briefly as follows: that ontogenetic adaptation is a very profound character. It enables animals and plants to survive very critical changes in their environment. Thus all the individuals of a race are similarly modified over such long periods of time that very gradually congenital or phylogenetic variations which happen to coincide with the ontogenetic adaptive variations, are selected. Thus there would result an apparent, but not real transmission of acquired characters.

This hypothesis, if it has no limitations, brings about a very unexpected harmony between the Lamarckian and Darwinian aspects of evolution, by mutual concessions upon the part of the adherents of both theories. While it abandons the transmission of acquired characters, it places individual adaptation first, and fortuitous variation second; as Lamarckians have always contended, instead of placing survival conditioned by fortuitous variations first and foremost, as selectionists have contended.

This hypothesis has been endorsed by Alfred Wallace. It appears to the author, however, that it is subject to limitations and exceptions which go far to nullify its universal application. This is especially seen in the fact that the law of Determinate Variation is observed to operate with equal force in certain structures, such as the teeth, which are not improved by individual use or exercise, as strongly as in structures which are so improved. A very large class of determinate variations in other stationary characters, such as the inner parts of the skull, also remain unexplained. The author's studies of teeth in a great many phyla of Mammalia in past time have convinced him that there are fundamental predispositions to vary in certain directions; that the evolution of the teeth is marked out beforehand by hereditary influences which extend back hundreds of thousands of years. These predispositions are aroused under certain exciting causes and the progress of teeth development takes a certain form converting into actuality what has hitherto been potentiality.

This paper was discussed by Dr. Theodore Gill and Prof. Edward D. Campbell.

(Abstract of Professor POULTON'S paper.)

IT must be admitted that the adaptation of the individual to its environment during its own lifetime possesses all the significance attributed to it by Professor Osborn, Professor Baldwin, and Professor Lloyd Mor-

gan. These authorities justly claim that the power of the individual to play a certain part in the struggle for life may constantly give a definite trend and direction to evolution, and that although the results of a purely individual response to external forces are not hereditary, yet indirectly they may result in the permanent addition of corresponding powers to the species; — inasmuch as they may render possible the operation of natural selection in perpetuating and increasing those inherent hereditary variations which go further in the same direction than the powers which are confined to the individual.

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Professor Osborn's metaphor in opening this discussion puts the matter quite clearly and will be at once accepted by all Darwinians. If the human species were led by fear of enemies or want of food to adopt an arboreal life, all the powers of purely individual adaptation would be at once employed in this direction and would produce considerable individual effects. In fact the adoption of such a mode of life would at first depend on the existence of such powers. In this way natural selection would be compelled to act along a certain path, and would be given time in which to produce hereditary changes in the direction of fitness for arboreal life. These changes would probably at first be chiefly functional, as Mr. Cunningham has argued (in the Preface to his Translation of Eimer). On these principles we can understand the arboreal kangaroo (Dendrolagus) found in certain islands of the Malay Archipelago, which is apparently but slightly altered from the terrestrial forms found in Australia. Professor Osborn has alluded to the arboreal habits said to have been recently acquired by Australian rabbits; these and the similar modification in habits of West Indian rats, are further examples of individual adaptive modification which may well become the starting point (in the sense applied above) of specific variation led by natural selection in the definite direction of more and more complete adjustment to the necessities of arboreal life. Although this conclusion seems to me to be clear and sound, and the principles involved a substantial gain in the attempt to understand the motive forces by which the great process of organic evolution has been brought about, I cannot admit that the importance of natural selection is in any way diminished. I do not believe that these important principles form any real compromise between the Lamarckian and Darwinian positions, in the sense of an equal surrender on either side, and the adoption of an intermediate position. The surrender of the Lamarckian position seems to me complete, while the considerations now advanced only confer added significance and strength to the Darwinian standpoint.

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I propose to devote the remainder of the time at my disposal in support of the conclusion that the power of individual adaptation possessed by the organism forms one of the highest achievements of natural selection and cannot in any true sense be considered as its substitute. Professor Baldwin and Prof. Lloyd Morgan thoroughly agree with this conclusion

and have enforced it in their writings on the subject of organic selection. The contention here urged is that natural selection works upon the highest organisms in such a way that they have become modifiable, and that this power of purely individual adaptability in fact acts as the nurse by whose help the species, as the above named authorities maintain, can live through times in which the needed inherent variations are not forthcoming, but in part acts also as a substitute, not indeed for natural selection, but for the ordinary operation by which the latter produces change. In this latter case natural selection acts so as to produce a plastic adaptable individual which can meet any of the various forces to which it is likely to be exposed by producing the appropriate modification and this, it is claimed, is in many instances more valuable than the more perfect, but more rigid, adjustment of inherent variations to a fixed set of conditions.

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A good example of the eminent advantages of adaptability in many directions, over accurate adjustment in fewer directions, is to be found in a comparison between the higher parts of the nervous system in insects and birds. The insect performs its various actions instinctively and perfectly from the first; it is almost incapable of education and of modifying its actions as the result of the observation of the effects of some new danger. It would appear that the introduction of the electric light can only affect the insects which are most attracted to it, by the gradual operation of natural selection. In the clothes-moths, which infest our houses, we may see an example of this; for these insects seem to be comparatively indifferent to light. Birds, on the other hand, have the power of learning from experience, of reasoning from the results of observation. At first terrified by railway trains they learn that they are not dangerous, and cease to be alarmed; while the effect of firearms results in their increased wariness.

If this view of individual adaptability as due to natural selection be not accepted it may be supposed that the individual modifications are due either to the direct action of the external forces, or to the tendencies of the organism. But it is impossible to understand how the mechanical operation of such forces as pressure, friction, stress, etc., continued through a lifetime, could evoke useful responses, or why the response should just attain and then be arrested at a level of maximum efficiency. The other supposition, that organisms are so constituted that they *must* react under external stimuli by the production of new, useful characters, or the useful modification of old ones, seems to me to be essentially the same as the old "innate tendency toward perfection" as the motive cause of evolution — a conception which is not much more satisfactory than special creation itself. The inadequacy of the view is clearly shown when we consider that the external forces which awake response in an organism generally belong to its inorganic (physical or chemical) environment, while the usefulness of the response has relation to its organic environment (enemies, prey, etc.). Thus one set of forces supply the stimuli which evoke a response to another and very different set of forces. We can therefore

accept neither of the suggestions which have been offered. Useful individual modifications are not directly due to the external forces and are not due to the inherent constitution of the organism.

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The only remaining hypothesis is that which I have already mentioned — the view that whenever organisms react adaptively under external forces they do so because of special powers conferred on them by natural selection. This hypothesis will, it seems to me, meet and satisfactorily explain all the facts of the case — whether employed as a preparation or as a substitute for hereditary variations accumulated by Natural Selection.

In closing the discussion, in reply to Professor Poulton, Professor Osborn stated that natural selectionists were wont to embrace every new process discovered in nature in the old comprehensive power of selection; but that he believed that plastic adaptation had not been acquired by Natural Selection but was a fundamental property of protoplasm.

MIMICRY IN BUTTERFLIES OF THE GENUS HYPOLIMNAS AND ITS BEARING ON OLDER AND MORE RECENT THEORIES OF MIMICRY. By Prof. EDWARD B. POULTON, M.A., F.R.S., University of Oxford, England.

THE theory of mimicry suggested by H. W. Bates in 1862 explained the superficial resemblance of a rare to a common species in the same locality by supposing that the latter possessed some special means of defence (such as unpleasant taste, smell, etc.), and that the former, without the special defence, was mistaken by enemies for the latter, and thus escaped a considerable amount of persecution. The relation may be compared to that existing between a successful well-known firm and another small unscrupulous one which lives upon its reputation. On the other hand, Bates thoroughly recognized the existence of resemblance between the specially defended forms themselves. These he could not explain by his theory of mimicry, and suggested that they were a result of the influence of locality. Many years later Fritz Müller satisfactorily explained this difficulty by suggesting that a common type of appearance simplified the education of enemies and thus was the means of saving life The lives of many individuals must be sacrificed before enemies have learned to recognize and to avoid the colors and patterns which indicate some special means of defence, and the fewer such patterns in any locality the smaller the sacrifice. The relation may be compared to that between two successful firms which combine to use a common advertisement.

This latter theory, although received rather coldly at first, has gradually made way, and seems now likely to occupy a good deal of the ground

formerly believed to be covered by the former theory. Thus, Dr. F. A. Dixey, of Oxford, has recently shown that South American *Heliconinæ* are affected by the color of certain *Pierinæ* which have hitherto been looked upon as true Batesian mimics of the former.

The old-world nymphaline genus *Hypolimnas* has been regarded as one of the best examples of mimicry, but an unbiassed examination leads to the opinion that it affords a case of Müllerian rather than Batesian resemblance.

In India the female of the common species *H. bolina* resembles *Euplœa core* while the male is a dark butterfly with a large white spot, shot with blue, on each of the four wings. Throughout the Malay Archipelago representative species occur with males like that of *H. bolina* and females resembling the local Euplœas. Occasionally, as in Ke Island and the Solomons, species of the genus occur in which the male as well as the female resembles a *Euplœa*. In Fiji the male is as in the Indian species while the female is extremely variable, ranging from forms like the male through intermediate varieties, to be brown and straw-colored individuals. The Euplœas of Fiji are not sufficiently known, but it is very improbable that all the forms of the female *Hypolimnas* are mimetic. A still more instructive case is that of the *nerina* form of female found, with a male like that of *H. bolina*, in Australia, Celebes, New Guinea, and other E. Indian Islands and in many of the Polynesian groups.

This conspicuous and abundant butterfly has, in addition to the four white-and-blue spots of the male, a large reddish brown patch upon each fore-wing. This well-marked form resembles no other butterfly except the *Danais chianippe* of Celebes, and, as this latter appears to be very rare, it is far more probable that the resemblance has come from the other side, and that the *Danais* has approached the *Hypolimnas*.

In Africa the sub-genus *Euralia* is represented by several species which resemble in both sexes species of the Ethiopian Danaine genus *Amauris*.

Finally there is the well-known and widespread *Hypolimnas missippus* which accompanies *Limnas chrysippus* throughout its range; while the female of the former resembles the latter very closely. In this case it is certain that we have to do with no struggling hard-pressed form, for the *Hypolimnas* has recently established itself in some of the West Indian Islands and in Demerara — localities in which its model, *L. chrysippus*, is as yet unknown.

To sum up — the genus *Hypolimnas* is distinguished among nymphaline genera for the extent to which its numerous and widespread species resemble the local distasteful forms of Euplœinæ or Danainæ.

Upon the older theory of Bates this would be explained by supposing that the genus is very hard-pressed in the struggle and has thus been driven to mimicry almost everywhere. Upon the newer Müllerian theory it is supposed that the genus is distinguished among nymphaline genera by some special defence, probably in the way of taste or smell or indigestibility, and that it has been to its advantage to adopt the advertise-

ment of still better-known and probably still more distasteful forms in its locality.

The abundance of the various species, the conspicuous *nerina* form of female, and the resemblance of a rare Danaid to it, the recent spread of *H. misippus* beyond the limits of its model all support this latter interpretation.

MIMICRY AS EVIDENCE FOR THE ANCESTRAL HOME OF A WIDE-RANGING SPECIES. By Prof. EDWARD B. POULTON, M.A., F.R.S., University of Oxford, England.

WE know on historic evidence that *Anosia plexippus* has spread and is spreading through the warmer parts of the world. If we had not this evidence, it might still be inferred with safety that North America is the ancestral home as compared with the other countries now inhabited by this species. In North America we find a very perfect mimic in *Limenitis misippus*, while in no other country has the occupation been long enough to permit of such a resemblance growing up.

Similar evidence indicates that Africa is the ancestral home of *Limnas chrysippus* which now ranges through almost all the warmer parts of the Old World. The far greater effect which has been produced by this species upon the Lepidopterous fauna of Africa as compared with that of other lands, proves a far longer sojourn in the former.

A METHOD OF LABELLING TYPE SPECIMENS IN COLLECTIONS OF INSECTS. By Prof. EDWARD B. POULTON, M.A., F.R.S., University of Oxford. England.

THE method adopted in the Hope Department of the University Museum, Oxford, consists in printing a form of type label to which the reference to the author's description of the species can be added. Such a label is placed on the pin, below the specimen, while a duplicate label is pinned beside it, in the most convenient position for the student. All essential facts are recorded on this label and a second one stating the locality, date of capture, name of captor, and date of presentation. This latter (which is prepared for *all* specimens recently added to the collections) is similarly placed beside as well as on the insect. Ordinary printers' ink is used for both labels because of its permanence; but the type labels are distinguished by a red line just within the margin.

Geographical distribution of the golden warblers. By Harry C. Oberholser, U. S. Department of Agriculture, Washington, D. C.

The so-called golden warblers form a group of some twenty-five species and subspecies in the genus *Dendroica*, and are distributed over almost the entire Nearctic region, together with the Antillean, Columbian and Central American subregions of the Neotropical. The greatest differentiation of forms occurs in the West Indies, where the distribution of many of the species is somewhat anomalous.

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ADDRESS

BY

GEORGE F. ATKINSON,

VICE PRESIDENT, AND CHAIRMAN OF SECTION G.

EXPERIMENTAL MORPHOLOGY.

In looking at the progress which has been made in the study of plant morphology, I have been as much impressed with the different attitudes of mind toward the subject during the past one hundred and fifty years, as by the advance which has taken place in methods of study and the important acquisitions to botanical science. These different view points have coincided to some extent with distinct periods of time. What Sachs, in his "history of botany," calls the "new morphology" was ushered in near the middle of the present century by Von Mohl's researches in anatomy, by Naegeli's investigations of the cell and Schleiden's history of the development of the flower. The leading idea in the study of morphology, during this period, was the inductive method for the purpose of discerning fundamental principles and laws, not simply the establishment of individual facts, which was especially characteristic of the earlier period when the dogma of the constancy of species prevailed.

The work of the "herbalists" had paved the way for the more logical study of plant members by increasing a knowledge of species, though their work speedily degenerated into mere collections of material and tabulations of species with inadequate descriptions. Later, the advocates of metamorphosis and spiral growth had given an impetus more to the study of nature, though diluted with much poetry and too largely subservient to the imagination, and to preconceived or idealistic notions.

But it was reserved for Hoffmeister ('59), whose work followed within three decades of the beginnings of this period, to add to the inductive method, as now laid down, the comparative method; and extending his researches down into the Pteridophyta and Bryophyta, he not only established for these groups facts in sexuality which Camerarius and Robert Brown had done for the Spermatophyta, but he did it in a far superior manner. He thus laid the foundation for our present conceptions of the comparative morphology of plants. Naegeli's investigations of the cell had emphasized the importance of its study in development, and now the relation of cell growth to the form of the plant was carried to a high degree, and an attempt was made to show how dependent the form of the plant was on the growth of the apical cell in the Pteridophyta and Bryophyta, though later researches have modified this view; and how necessary a knowledge of the sequence of cell division was to an understanding of homologies and relationships. Thus in developmental and comparative studies morphology has been placed on a broader and more natural basis, and the homologies and relationships of organs between the lower and higher plants are better understood.

But the growth of comparative morphology has been accompanied by the interpretation of structures usually from a teleological standpoint, and in many cases with the innate propensity of the mind to look at nature in the light of the old idealistic theories of metamorphosis.

I wish now to enquire if we have not recently entered upon a new period in our study of comparative morphology. There are many important questions on which comparative studies of development, under natural or normal conditions alone, cannot afford a sufficient number of data. We are constantly confronted with the problems of the interpretation of structure and form, not only as to how it stands in relation to structures in other plants, which we deal with in comparative morphology, but the meaning of the structure or form itself, and in relation to the other structures of the organism, in relation to the environment, and in relation to the past. This must be met by an enquiry on our part as to why the structure or form is what it is, and what are the conditions which influence it. This we are accustomed to do by *experiment*, and it begins to appear that our final judgments upon many questions of morphology, especially those which relate to variation, homology,

etc., must be formed after the evidence is obtained in this higher trial court, that of *experimental morphology*. While experimental morphology, as a designation of one branch of research in plants, or as a distinct and important field of study, is not yet fully taken cognizance of by botanists, we have only to consult our recent literature to find evidence that this great and little explored field has already been entered upon.

Experimental methods of research in the study of plants have been in vogue for some time, but chiefly by plant physiologists and largely from the standpoint of the physical and chemical activities of the plant, as well as those phases of nutrition and irritability, and of histologic structure, which relate largely to the life processes of the plant, and in which the physiologist is therefore mainly interested. In recent years there has been a tendency in physiological research to limit the special scope of these investigations to those subjects of a physical and chemical nature. At the same time, the study of the structure and behavior of protoplasm is coming to be regarded as a morphological one; and, while experimental methods of research as applied to the morphology of protoplasm and the cell are comparatively new, there is already a considerable literature on the subject even from the side of plant organisms (Davenport, '97). While certain of the phenomena of irritability and growth are closely related to the physics of plant life, they are essentially morphologic, and it is here especially that we have a voluminous literature based strictly on the inductions gained by experimentation, and for which we have chiefly to thank the plant physiologist.

If we were to write the full history of experimental morphology in its broadest aspects, we could not omit those important experimental researches on the lower plants in determining the ontogeny of polymorphic species among the algæ and fungi which were begun so ably by De Bary, Tulasne, Pringsheim and others, and carried on by a host of European and American botanists. The tone which these investigations gave to taxonomic botany has been felt in the study of the higher plants, by using to some extent the opportunities at botanic gardens, where plants of a group may be grown under similar conditions for comparison, and in the establishment of alpine, subalpine and tropical stations for the purpose of studying the influence of climate on the form and variations of plants, and in studying the effect of varying external conditions.

While experimental morphology in its broadest sense also includes in its domain cellular morphology and the changes resulting from the directive or taxic forces accompanying growth, it is not these phases of morphology with which I wish to deal here.

The question is rather that of experimental morphology as applied to the interpretation of the modes of progress followed by members and organs in attaining their morphologic individuality, in the tracing of homologies, in the relation of members associated by antagonistic or correlative forces, the dependence of diversity of function in homologous members on external and internal forces, as well as the causes which determine the character of certain paternal or maternal structures. I shall deal more especially with the experimental evidence touching the relation of the members of the plant which has been represented under the concept of the leaf, as expressed in the metamorphosis theory of the idealistic morphology. The poetry and mystery of the plant world, which were so beautifully set forth in the writings of Goethe and A. Braun, are interesting and entrancing; and poetic communication with nature is elevating to our ethical and spiritual natures. But fancy or poetry cannot guide us safely to the court of enquiry. We must sometimes lay these instincts aside and deal with nature in a cold, experimental, calculating spirit.

The beginnings of experimental morphology were made about one century ago when Knight, celebrated also for the impulse which he gave to experimental physiology, performed some very simple experiments on the potato plant. The underground shoots and tubers had been called roots until Hunter ('77) pointed out the fact that they were similar to stems. Knight tested the matter by experiment, and demonstrated that the tubers and underground stems could be made to grow into aërial leafy shoots. This he regarded as indicating a compensation of growth, and he thought further that a compensation of growth could be shown to exist between the production of tubers and flowers on the potato plant. He reasoned that by the prevention of the development of the tubers, the plant might be made to bloom. An early sort of potato was selected, one which rarely or never set flowers, and the shoots were potted with the earth well heaped up into a mound around the end of the shoot. When growth was well started, the soil was washed away from the shoot and the upper part of the roots, so that the plant was only connected with the soil by the roots. The tubers were

prevented from growing and numbers of flowers were formed. This result he also looked upon as indicating a compensation of growth between the flowers and tubers. While we recognize Knight's experiments as of great importance, yet he erred in his interpretation of the results of this supposed correlation between the tubers and flowers, as Vöchting ('87, '95) has shown. By repeating Knight's experiment, and also by growing shoots so that tubers would be prevented from developing, while at the same time the roots would be protected, flowers were obtained in the first case while they were not in the second; so that the compensation of growth, or correlation of growth here, exists between the vegetative portion of the plant and the flowers, instead of between the production of tubers and flowers, as Knight supposed.

The theory of metamorphosis as expressed by Goethe and A. Braun ('51), and applied to the leaf, regarded the leaf as a *concept* or *idea*. As Goebel ('80) points out, Braun did not look upon any one form as the typical one which, through transformation, had developed the various leaf forms, but each one represented a wave in the march of the successive billows of a metamorphosis; the shoot manifesting successive repetitions or renewals of growth each season, representing in order the "niederblätter, laubblätter, hochblätter, kelchblätter, blumenblätter, staubblätter, fruchtblätter." Though it had been since suggested from time to time, as Goebel ('80) remarks, that the foliage leaf must be regarded as the original one from which all the other forms had arisen (though at that time Goebel did not think this the correct view), no research, he says, had been carried on, not even in a single case, to determine this point. Goebel plainly showed in the case of *Prunus padus* that axillary buds, which, under normal conditions, were formed one year with several bud scales, could be made by artificial treatment to develop during the first year. This he accomplished by removing all the leaves from small trees in April, and in some cases also cutting away the terminal shoot. In these cases the axillary shoot, instead of developing buds which remained dormant for one year, as in normal cases, at once began to grow and developed well formed shoots. Instead of the usual number of bud scales, there were first two stipule-like outgrowths, and then fully expanded leaves were formed; so that in this case, he says, the metamorphosis of the leaf to bud scales was prevented. For this relation of bud scales to foliage leaves Goebel proposed the term, "correla-

tion of growth" ('80). In the case of *Vicia faba*, removal of the lamina of the leaf of seedlings when it was very young, caused the stipules to attain a large size and to perform the function of the assimilating leaf. He here points out that experimentation aids us in interpreting certain morphological phenomena which otherwise might remain obscure. He cites the occasional occurrence (Moquin-Tandon) in the open, of enlarged stipules of this plant which his experiment aids in interpreting. In the case of *Lathyrus aphaca* the stipules are large and leaflike, while the part which corresponds to the lamina of the leaf is in the form of a tendril, the correlation processes here having brought about the enlargement of the stipules as the lamina of the leaf became adapted to another function. Kronfeld ('86, '87) repeated some of Goebel's experiments, obtaining the same results and extended them to other plants (*Pirus malus* and *Pisum sativum*), while negative results attended some other experiments. Hildebrand ('92), in some experiments on seedlings and cuttings, found that external influences affected the leaves, and in some cases where the cotyledons were cut, foliage leaves appeared in place of the usual bud scales, and in *Oxalis rubella* removal of the foliage leaf, which appears after the cotyledons, caused the first of the bulb scales, which normally follow the foliage leaf, to expand into a foliage leaf.

In some experiments on the influence of light on the form of the leaves Goebel ('96) has obtained some interesting results. Plants of *Campanula rotundifolia* were used. In this species the lower leaves are petioled and possess broadly expanded heart-shaped laminæ, while the upper leaves are narrow and sessile with intergrading forms. Plants in different stages of growth were placed in a poorly lighted room. Young plants which had only the round leaves, under these conditions continued to develop only this form of leaf; while older plants, which had both kinds of leaves when the experiment was started, now developed on the new growth of the shoot the round-leaved form. In the case of plants on which the flower shoot had already developed, side shoots with the round leaves were formed. Excluding the possibility of other conditions having an influence here, the changes in the form of the leaves have been shown to be due to a varying intensity of light. The situation of the plants in the open favors this view, since the leaves near the ground in these places are not so well lighted as the leaves higher up on the stem. In this case the effect of dampness is not

taken into account by the experimenter, and since dampness does have an influence on the size of the leaf it would seem that it might be at least one of the factors here. An attempt was now made to prevent the development of the round leaves on the young seedlings. For this purpose the germinating seeds were kept under the influence of strong and continuous lighting. The round leaves were nevertheless developed in the early stage, an indication that this form of the leaf on the seedling has become fixed and is hereditary. Hering ('96) found that enclosing the larger cotyledon of Streptocarpus in a plaster cast, so as to check the growth, the smaller and usually fugacious one grew to the size of the large one, provided the experiment was started before the small one was too old. Amputation of the large cotyledon gave the same results.

Other experimenters have directed their attention to the effect of light and gravity on the arrangement of the leaves on the stem, as well as the effect of light on the length of the petioles and breadth of the lamina. Among these may be mentioned the work of Weisse ('94, '95), Rosenvinge ('88) and others.

Goebel has shown experimentally that dampness is also one of the external influences which can change the character of xerophyllous leaves. A New Zealand species of Veronica of xerophyllous habit and scaly appressed leaves, in the seedling stage has spreading leaves with a broad lamina. Older plants can be forced into this condition in which the leaves are expanded by growing them in a moist vessel ('96). Gain, Askenasy and others have shown that dampness or dryness has an important influence in determining the character of the leaves.

The results of the experiments in showing the relation of the leaf to the bud scales Goebel regards as evidence that the foliage leaf is the original form of the two, and that the bud scale is a modification of it.

Treub ('72) conducted some interesting experiments for the purpose of determining the homology of the pappus of the Compositæ. Gall-insects were employed to stimulate the pappus of *Hieracium umbellatum* and it was made to grow into a normal calyx with five lobes. (A recent letter from Professor Treub states that he later repeated these experiments with other species of Compositæ with like results but the work was not published.) Kny ('94) found, in seedlings and cuttings, which he experimented with

that, while there was still stored food available for the roots and shoots, there was little if any dependence of one upon the other. Hering ('96) comes to somewhat different conclusions as a result of his experiments, finding that in some cases there was a slight increase of growth, while in others, growth of the one was reciprocally retarded, when the other was checked in development. Numerous cases of horticultural practice in pollination of fruits shows that the form and size of the fruit and of the adjacent parts, as well as the longer or shorter period of existence of the floral envelopes, can be influenced by pollination.

The investigations carried on by Klebs ('96) in the conjugation of Spirogyra suggest how experimentation of this kind may be utilized to determine questions which, in special cases, cannot be arrived at easily by direct investigation. If threads of *Spirogyra varians* which are ready for conjugation are brought into a (0.5%) solution of agar-agar, in such a way that nearly parallel threads lie at a varying distance in their windings, where they are within certain limits the conjugation tubes are developed and the zygospores are formed. But where the threads lie at too great a distance for the influences to be exerted, the cells remain sterile, and no conjugation tubes are developed. If now these threads be brought into a nutrient solution, the cells which were compelled to remain sterile grow and develop into new threads, *i. e.* they take on the vegetative, though they are fully prepared for the sexual function. Strasburger ('97) has pointed out that this may be taken as excluding the possibility of there being a reducing division of the chromosomes during the maturing of the sexual cells, a process which takes place in animals, and that the behavior of Spirogyra in this respect agrees with what is known to take place in the higher plants, viz., that the reduction process is not one which is concerned in the maturity of the gametes. The same could be said of Polyphagus in which Nowakowski ('78) found that, before the zygospore was completely formed, the protoplasm moved out and formed a new sporangium.

In *Protosiphon botryoides* Klebs was also able to compel the parthenogenetic development of the motile gametes, and the same thing was observed in the case of the gametes of Ulothrix. If we are justified in interpreting this phenomenon as Strasburger suggests, the evidence which Raciborski ('96) gives as a result of his experiments with *Basidiobolus ranarum* would support the idea

that there is no reducing division in the chromosomes before the formation of the nuclei of the gametes. Raciborski found that in the case of the young zygospores of this plant, in old nutrient medium where the fusion of the plasma contents had taken place, but before the nuclei had fused, if they were placed in a fresh nutrient medium the fusion of the nuclei was prevented, and vegetative growth took place forming a hypha which possessed two nuclei, the paternal one and the maternal one. Raciborski interprets Eidam's ('87) study of the nuclear division prior to the copulation of the gametes as showing that the reducing division takes place here as in the maturation of the sexual cells of animals, and looks upon the premature germination of the zygospore as showing that a paternal and maternal nucleus possesses the full peculiarities of a normal vegetative one. However we are not justified in claiming a reducing division for the nuclei preceding the formation of the gametes in Basidiobolus from the work of Eidam, since he was not able to obtain sufficiently clear figures of the division to determine definitely how many divisions took place, to say nothing of the lack of definite information as to the number of chromosomes. Fairchild ('96) has recently studied more carefully the nuclear division, but on account of the large number of the chromosomes was not able to determine whether a reduction takes place. He points out, as others have done, the similarity in the process of the formation of the conjugating cells of Basidiobolus and Mougeotia among the Mesocarpeæ, and to these there might be added the case of Sirogonium in which the paternal cell just prior to copulation undergoes division. The division of the copulation cells in Basidiobolus, Mougeotia, Sirogonium, etc., suggest at least some sort of preparatory act, but whether this is for the purpose of a quantitative reduction of the kinoplasm as Strasburger thinks sometimes takes place, or is a real reduction in the number of the chromosomes, must be determined by further study, so that the bearing of these experiments on the question of a reducing division must for the time be held in reserve.

One of the very interesting fields for experimental investigation is that upon the correlation processes which govern the morphology of the sporophylls (stamens and pistils) of the Spermatophyta. One of the controlling influences seems to be that of nutrition, and in this respect there is some comparison to be made with the correlative processes which govern the determination of sex in

plants. Among the ferns and some others of the Pteridophyta a number of experiments have been carried on by Prantl ('78), Bauke, Heim ('96), Buchtien ('87), and others to determine the conditions which influence the development of antheridia and archegonia. Prantl found that, on prothallia of the ferns grown in solutions lacking nitrogen, there was no meristem and consequently no archegonia, while antheridia were developed; but, if the prothallia were changed to solutions containing nitrogen, meristem and archegonia were developed. All the experiments agree in respect to nutrition; with scanty nutrition, antheridia only were developed, while with abundant nutriment archegonia were also developed. Heim studied the influence of light and found that fern prothallia grow best with light of 20%–25%. Exclusion of the ultra violet rays does not affect the development of the sexual organs. He argues from this that the ultra violet rays are not concerned in the elaboration of the material for flower production as Sachs had suggested. In yellow light the prothallia grew little in breadth; they also grew upward, so that few of the rhizoids could reach the substratum. Antheridia were here very numerous. After seven months these prothallia were changed to normal light, and in four months afterward archegonia were developed.

Among the algæ Klebs ('96) has experimented especially with Vaucheria, such species as *V. repens* and *V. ornithocephala*, where the antheridia and oögonia are developed near each other on the same thread. With weak light, especially artificial light, the oögonium begins first to degenerate. He never succeeded in suppressing the antheridia and at the same time to produce oögonia.

High temperature, low air pressure or weak light tend to suppress the oögonia, and at the same time the antheridia may increase so that the number in a group is quite large, while the oögonium degenerates or develops vegetatively. Klebs concludes from his experiments that the causes which lie at the bottom of the origin of sex in Vaucheria, as in other organisms, are shrouded in the deepest mystery. In the higher plants a number of experiments have been carried on for the purpose of learning the conditions which govern the production of staminate and pistillate flowers, or in other words the two kinds of sporophylls. From numerous empirical observations on diœcious Spermatophyta, the inference has generally been drawn that nutrition bears an important relation to the development of the staminate and pistillate

flowers; that scanty nutrition produces a preponderance of staminate plants, while an abundance of nutrition produces a preponderance of pistillate plants. For a period covering three decades several investigators have dealt with this question experimentally, notably K. Müller ('64), Haberlandt ('75, '77), and Hoffmann ('85). These experiments in general give some support to the inferences from observation, yet the results indicate that other influences are also at work, for the ratios of preponderance either way are not large enough to argue for this influence alone. In a majority of cases thick sowings, which in reality correspond to scanty nutrition, tend to produce staminate plants; while thin sowings tend to produce pistillate plants. In the case of the hemp (*Cannabis sativa*), Hoffmann found that these conditions had practically no influence. He suggests that the character of each may have been fixed during the development of the seed, or even that it may be due to late or early fecundation ('71).

In monœcious plants it has often been observed that pistillate flowers change to staminate ones and *vice versa*, and in diœcious plants pistillate ones sometimes are observed to change to staminate ones (the hemp for example, see Nagel 1879). K. Müller ('64) states that, by scanty nutrition, the pistillate flowers of *Zea mays* can be reduced to staminate ones.

Among the pines what are called androgynous cones have in some instances been observed. In *Pinus rigida* and *P. thunbergii*, for example, they occur (Masters). Matsuda ('92) has described in the case of *Pinus densiflora* of Japan, pistillate and androgynous flowers which developed in place of the staminate flowers and, conversely, staminate and androgynous flowers in place of pistillate ones. Fujii ('95) has observed that where the pistillate or androgynous flowers of *Pinus densiflora* occur in place of the staminate ones, they are usually limited to the long shoots which are developed from the short ones of the previous year. The proximity of these transformed short shoots (Kurztriebe) to injuries of the long ones, suggested that the cutting away of the long ones might induce the short ones to develop into long ones, and the flowers which were in the position for staminate ones to become pistillate.

Fujii says "In fact, the injuries producing such effect are frequently given by Japanese gardeners to the shoots of the year of *Pinus densiflora* in their operations of annual pollarding. But the 'Langtrieb' which is transformed from a 'Kurztrieb' of the last

year does not necessarily bear female of hermaphrodite flowers in the positions of male flowers." To determine the influence of pollarding the shoots, he carried on experiments on this pine in the spring of 1895. He pollarded the shoots, so as, he terms it, to induce the nourishment to be employed in the development of the flowers and short shoots near the seat of injury; in other cases, one or two shoots were preserved, while all the adjacent shoots of last year's growth at the top of the branch were removed and, further, both of these processes were combined. Out of the 45 branches experimented on, and on which there were no signs of previous injury, there were 9 pistillate or androgynous flowers in place of staminate ones; in 21 branches with signs of previous injury, 5 were transformed; while in 2283, not experimented on and with no signs of previous injury, only 7 were transformed. Such abnormal flowers then are due, largely, to the injuries upon the adjacent shoots, and Fujii thinks largely to the increased amount of nourishment which is conveyed to them as a result of this.

From the experiments thus far conducted upon the determination of sex in plants or upon the determination of staminate or pistillate members of the flower, nutrition has at least some influence in building up the nourishing tissue for the two different organs or members. This can in part be explained on the ground that antheridia and staminate members of the plant are more or less short lived in comparison with the archegonia and pistillate members, the latter requiring more bulk of tissue to serve the purpose of protection and nourishment to the egg and embryo. It is thus evident that while some progress has been made in the study of this question we are far from a solution of it. Experiment has proceeded largely from a single standpoint, viz., that of the influence of nutrition. Other factors should be taken into consideration, for there are evidently other external influences and internal forces which play an important rôle, as well as certain correlation processes, perhaps connected with the osmotic activities of the cell sap.

The relation of the parts of the flower to the foliage leaves is a subject which has from time to time called forth discussion. That they are but modifications of the foliage leaf, or constituents of the leaf concept, is the contention of the metamorphosis theory, and that the so-called sporophylls are modified foliage leaves is accepted with little hesitation by nearly all botanists, though it would be

very difficult it seems to me, for any one to present any very strong argument from a phylogenetic standpoint in favor of the foliage leaf being the primary form in its evolution on the sporophyte, and that the sporophyll is a modern adaptation of the foliage leaf. Numerous cases are known of intermediate forms between sporophylls and foliage leaves both in the Spermatophyta and Pteridophyta. These are sometimes regarded as showing reversion, or indicating atavism, or in the case of some of the ferns as being contracted and partially fertile conditions of the foliage leaf. There has been a great deal of speculation regarding these interesting abnormal forms, but very little experimentation to determine the causes or conditions which govern the processes.

In 1894 I succeeded in producing a large series of these intermediate forms in the sensitive fern (*Onoclea sensibilis*). The experiments were carried on at the time for the especial purpose of determining whether in this species the partially developed sporophyll could be made to change to a foliage leaf, and yet possess characters which would identify it as a transformed sporophyll. The experiments were carried on where there were a large number of the fern plants. When the first foliage leaves were about 25cm. high they were cut away (about the middle of May). The second crop of foliage leaves was also cut away when they were about the same height during the month of June. During July, when the uninjured ferns were developing the normal sporophylls, those which were experimented upon presented a large series of gradations between the normal sporophyll and fully expanded foliage leaves. Among these examples there are all intermediate stages from sporophylls which show very slight expansions of the distal portion of the sporophyll and the distal portions of the pinnæ, until we reach forms which it is very difficult to distinguish from the normal foliage leaf. Accompanying these changes are all stages in the sterilization of the sporangia (and the formation of prothalloid growths) on the more broadly expanded sporophylls there being only faint evidences of the indusia.

The following year (1895), similar experiments were carried on with the ostrich fern (*Onoclea struthiopteris*) and similar results were obtained. At the time that these experiments were conducted, I was unaware of the experiments performed by Goebel ('87), on the ostrich fern. The results he reached were the same; the sporophyll was more or less completely transformed to a foliage leaf.

Goebel regards this as the result of the correlation processes, and looks upon it as indicating that the sporophyll is a transformed foliage leaf, and that the experiment proves the reality here of the modification which was suggested in the theory of metamorphosis, and thus the foliage leaf is looked upon by him as the primary form. Another interpretation has been given to these results, viz., that they strengthen the view that the sporophyll, from a phylogenetic standpoint, is primary, while the foliage leaf is secondary. What one interprets as a reversion, another regards as indicating a mode of progress in the sterilization of potentially sporogenous tissue, and its conversion into assimilatory tissue. It is perhaps rather to be explained by the adaptive equipoise of the correlative processes existing between the vegetative and fruiting portions of the plant, which is inherited from earliest times. Rather when spore production appears on the sporophyte could this process be looked upon as a reversion to the primary office of the sporophyte, so that in spore production of the higher plants we may have a constantly recurring reversion to a process which, in the remote past, was the sole function of this phase of the plant. In this way might be explained those cases where sporangia occur on the normal foliage leaf of Botrychium, and some peculiar cases which I have observed in *Osmunda cinnamomea*. In some of the examples of this species it would appear that growth of the leaf was marked by three different periods even after the fundament was outlined, the first a vegetative, second a spore producing, and third a vegetative again; for the basal portions of the leaf are expanded, the middle portions spore bearing, the passage into the middle portions being gradual so that many sporangia are on the margins of quite well developed pinnæ. These gradations of the basal part of the leaf, and their relation to the expanded vegetative basal portion, show that the transition here has been from partially formed foliage leaf to sporophyll after the fundament was established, and later the increments of the vegetative part from the middle toward the terminal portion, shown by the more and more expanded condition of the lamina and decreasing sporangia, which indicates that vegetative forces are again in the ascendency. This suggests how unstable is the poise between the vegetative leaf and sporophyll in structure and function in the case of this species.

For two successive years I have endeavored by experiment to produce this transformation in *Osmunda cinnamomea*, but thus far

without sufficiently marked results. The stem of the plant is stout, and this, together with the bases of the leaves closely overlapping, contains considerable amounts of stored nutriment which make it difficult to produce the results by simply cutting off the foliage leaves. The fact that these transformations are known to occur where fire has overspread the ground, and as I have observed where the logging in the woods seriously injured the stools of the plant, it would seem that deeper-seated injuries than the mere removal of foliage leaves would be required to produce the transformation in this species. It may be that such injury as results from fire or the severe crushing of the stools of the plant would be sufficient to disturb the equilibrium which existed at the time, that the action of the correlative forces is changed thereby, and there would be a tendency for the partially developed foliage leaves to form sporangia; then, when growth has proceeded for a time, this balance is again changed.

The theory that the foliage leaves of the sporophyte have been derived by a process of sterilization, and that the transformation of sporophylls to foliage leaves in an individual, indicates the mode of progress in this sterilization, does not necessarily involve the idea that the sporophyll of any of the ferns, as they now exist, was the primary form of the leaf in that species, and that by sterilization of some of the sporophylls, the present dimorphic form of the leaves was brought about. The process of the evolution of the leaf has probably been a gradual one and extends back to some ancestral form now totally unknown. One might differ from Professor Bower in the examples selected by him to illustrate the course of progress from a simple and slightly differentiated sporophyte to that exhibited in the various groups of the Pteridophyta. But it seems to me that he is right in so far as his contention for the evolution of vegetative and assimilatory members of the sporophyte, can be illustrated by a comparison of the different degrees of complexity represented by it in different groups, and that this illustrates the mode of progress, as he terms it, in the sterilization of potential sporogenous tissue.

On this point it appears that Professor Bower has been unjustly criticised. The forms selected to illustrate his theory were chosen not to represent ancestral forms, or direct phylogenetic lines, but solely for the purpose of illustrating the gradual transference of spore-bearing tissue from a central to a peripheral position, and

the gradual eruption and separation of spore-bearing areas, with the final sterilization of some of these outgrowths.

To maintain that in phylogeny the sporophyll is a transformed foliage leaf, would necessitate the predication of ancestral plants with only foliage leaves, and that in the case of these plants the vegetative condition of the sporophyte was the primary one, spore production being a later developed function. Of the forms below the Pteridophyta, so far as our present evidence goes, the sporophyte originated through what Bower calls the gradual elaboration of the zygote. All through the Bryophyta wherever a sporophyte is developed, spore production constantly recurs in each cycle of the development, and yet there is no indication of any foliar organs on the sporophyte. The simplest forms of the sporophyte contain no assimilatory tissue, but in the more complex forms assimilatory tissue is developed to some extent, showing that the correlative forces, which formerly were so balanced as to confine the vegetative growth to the gametophyte, and fruiting to the sporophyte, are later changing; that vegetative growth and assimilation are being transferred to the sporophyte; while the latter still retains the function of spore production, though postponed in the ontogeny of the plant.

If we cannot accept some such theory for the origin of sporophylls and foliage leaves, by gradual changes in potentially sporogenous tissue somewhat on the lines indicated by Bower, it seems to me it would be necessary, as already suggested, to predicate an ancestral form for the Pteridophyta in which spore production was absent. That is, spore production, in the sporophyte of ancestral forms of the Pteridophyta, may never have existed in the early period of its evolution, and spore production may have been a later development. But this, judging from the evidence which we have, is improbable, since the gametophyte alone would then be concerned in transmitting hereditary characters, unless the sporophyte through a long period developed the gametophyte stage through apospory. Bower says in taking issue with Goebel's statement that the experiments on Onoclea prove the sporophyll to be a transformed foliage leaf: "I assert, on the other hand, that this is not proved, and that a good case could be made out for priority of the sporophyte; in which event the conclusion would need to be inverted, *the foliage leaf would be looked upon as a sterilized sporophyll*. This would be perfectly consistent with the *correlation*

demonstrated by Professor Goebel's experiments, as also with the intercalation of a vegetative phase between the zygote and the production of spores." In another place he says: "To me, whether we take such simple cases as the Lycopods or the more complex case of the Filicineæ, the sporangium is not a gift showered by a bountiful providence upon preëxistent foliage leaves: the sporangium, like other parts, must be looked upon from the point of view of descent; its production in the individual or in the race may be deferred, owing to the intercalation of a vegetative phase, as above explained; while, in certain cases at least, we probably see in the foliage leaf the result of the sterilization of sporophylls. If this be so, much may be then said in favor of the view that the appearance of sporangia upon the later-formed leaves of the individual is a reversion to a more ancient type rather than a metamorphosis of a progressive order."

As I have endeavored to point out in another place ('96), if a disturbance of these correlative processes results in the transference of sporophyllary organs to vegetative ones on the sporophyte "why should there not be a similar influence brought to bear on the sporophyte, when the same function resides solely in the gametophyte, and a disturbing element of this kind is introduced? To me there are convincing grounds for believing that this influence was a very potent, though not the only one in the early evolution of sporophytic assimilatory organs. By this I do not mean that in the Bryophyta, for example, injury to the gametophyte would now produce distinct vegetative organs on the sporophyte, which would tend to make it independent of the gametophyte; but that, in the bryophtye-like ancestors of the pteridophytes, an influence of this kind did actually take place appears to me reasonable.

"In the gradual passage from an aquatic life, for which the gametophyte was better suited, to a terrestrial existence for which it was unadapted, a disturbance of the correlative processes was introduced. This would not only assist in the sterilization of some of the sporogenous tissue, which was taking place, but there would also be a tendency to force this function on some of the sterilized portions of the sporophyte, and to expand them into organs better adapted to this office. As eruptions in the mass of sporogenous tissue took place and sporophylls were evolved, this would be accompanied by the transference of the assimilatory function of the gametophyte to some of these sporophylls."

Because sporophytic vegetation is more suited to dry land conditions than the gametophytic vegetation, it has come to be the dominating feature of land areas. Because the sporophyte in the Pteridophyta and Spermatophyta leads an independent existence from the gametophyte, it must possess assimilatory tissue of its own, and this is necessarily developed first, in the ontogeny; but it does not necessarily follow, therefore, that the foliage leaf was the primary organ in the phylogeny of the sporophyte. The provision for the development of a large number of spores in the thallophytes, so that many may perish and still some remain to perpetuate the race, is laid hold on by the bryophytes where the mass of spore-bearing cells increases and becomes more stable, for purposes of the greatest importance. Instead of perishing, some of the sporogenous tissue forms protecting envelopes, then supporting and conducting tissue and, finally, in the pteridophytes and spermatophytes, nutritive and assimilatory structures are developed. Nature is prodigal in the production of initial elementary structures and organs; but, while making abundant provision for the life of the organism through the favored few, she has learned to turn an increasing number of the unfavored ones to good account. Acted upon by external agents and by internal forces, and a changing environment, advance is made, step by step, to higher, more stable and prolonged periods.

While we have not yet solved any one of these problems, the results of experimental morphology are sufficient to indicate the great importance of the subject and the need of fuller data from a much larger number of plants. If thus far the results of experiments have not been in all cases sufficient to overthrow the previous notions entertained touching the subjects involved, they at least show that there are good grounds for new thoughts and new interpretations, or for the amendment of the existing theories. While there is not time for detailing, even briefly, another line of experiment, viz., that upon leaf arrangement, I might simply call attention to the importance of the experiments conducted by Schumann ('89) and Weisse from the standpoint of Schwendener's mechanical theory of leaf arrangement ('78). Weisse ('94) shows that the validity of the so-called theory of the spiral arrangement of the leaves on the axis may be questioned, and there are good grounds for the opening of the discussion again. It seems to me, therefore, that the final judgment upon either side of all these questions cannot

now be given. It is for the purpose of bringing fresh to the minds of the working botanists the importance of the experimental method in dealing with these problems of nature, that this discussion is presented as a short contribution to the subject of experimental morphology of plants.

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[ABSTRACTS.]

TRILLIUM GRANDIFLORUM (MICHX.) SALISB.; ITS VARIATIONS, NORMAL AND TERATOLOGICAL. By Prof. CHARLES A. DAVIS, Alma College, Alma, Michigan.

THE variations from the typical plant, as described in the sixth edition of Gray's Manual of Botany, are such as are found in every plant, viz.: in shape of leaves and parts of the flower, and in the length of petioles, which are frequently one-half inch or more long, and of the pedicels. In teratological specimens, which are of very frequent occurrence, the type is departed from entirely and some or all the organs may be more or less changed in the same specimen. The most common change is that in which the petals are striped with green, varying from slight green veining to the entire disappearance of white. The green tissue is more compact than the white and there is frequently a notable alteration from the normal outline of the petals in such specimens. The striping of the petals is usually accompanied by a greater or less elongation of the petioles and pedicels and a decrease in size of the blades of the leaves and an entire change of their characteristic shape. In most cases also, there is a profound alteration in the essential organs of the flower, the stamens becoming much less freely polleniferous, with an increase in the thickness of the connective, while the pistils become sterile and bear no ovules, or revert entirely to the leaf form. The sepals are rarely affected by any change, but in one case examined they were all white and in several they were slightly stalked. The length of the petiole in teratological specimens varies from nothing to several inches and in one case the leaves were long petioled from the rootstock and in many the stem of the plant was reduced to the short portion under ground. Specimens amounting to about twenty per cent of those examined were entirely without leaves or with leaves represented by small bracts low down on the stem. In such specimens the sepals were usually larger than normal. The pedicel may be entirely wanting or may be several inches long from the rootstock. The stamens are least affected of all the organs, only about one per cent showing complete atrophy, all others from the specimens examined containing normal pollen but usually reduced in quantity. The pistils show great variety of change, the simplest being absence of ovules, the most common also. From this all gradations of alteration have been found to fully expanded green leaves.

Both pistils and stamens were frequently elongated and the ovary is sometimes long stalked and in place of ovules frequently contained small leaf-like bodies usually three in number rolled up inside of it. The carpels were frequently only partly united. Doubling of the floral organs was noted in a number of cases and in some both stamens and pistil were absent. The number of leaves in a whorl may be two or four throughout the entire plant. Doubling of the number of parts of all whorls was noted in some leafless specimens, the flower resembling a green rosette. In a single specimen the leaves were arranged in a spiral around the stem. More than four hundred of these specimens collected in a single locality have been examined and described and some of the most remarkable changes figured. The writer does not attempt to account for the peculiar instability of the species, but it does not seem to be pathological, for not rarely a double terminal bud of a single rootstock will produce one typical and one teratological specimen.

They may be attempts on the part of the plant to adjust itself to unsuitable environment or simply reversions to ancestral forms. The fact that fertile stamens are present in nearly all teratological specimens admits of the crossing of such forms with typical and normal forms and the perpetuation of them.

[This paper will be printed in the Bulletin of the Torrey Botanical Club.]

A DISCUSSION OF THE STRUCTURAL CHARACTERS OF THE ORDER PEZIZINEÆ OF SCHRŒTER. By DR. E. J. DURAND, Cornell University, Ithaca, N. Y.

THE structural characters used by Schrœter, Rehm and others as a basis for separating the order into families is discussed in the light of some recent studies by the author along the same lines.

THE TAXONOMIC VALUE OF FRUIT CHARACTERS IN THE GENUS GALIUM. By KARL M. WIEGAND, Cornell University, Ithaca, N. Y.

IT is natural to expect that orders closely related to the Umbelliferæ should also show considerable fruit variation among the genera at least and even among the species of the same genera. This is true of the genus *Galium*. Characters have already been found by means of which sub-groups in the genus can be established; but even the fruits of closely related species seem to show minor variations.

The fruits of Galium are bicarpellary and two-seeded. Each carpel is nearly distinct from its neighbor and closely invests the seed which it

contains. At maturity the carpellary coat is very thin, consisting of only three or four layers of cells. The most important structure from a taxonomic point of view is the peculiar seed. This consists of a dense horny endosperm in which is imbedded the slender, but comparatively well developed embryo. It is in the endosperm that we find the greatest variation. This in general is saucer-shaped or cup-shaped with the opening toward the axis of the fruit. In *G. trifidum* and *G. tinctorium*, the edges of this cup are still farther extended until they almost meet, thus forming a spherical endosperm entirely enclosing the space within except for a minute pore at the inner angle by means of which communication is established with the exterior. In *G. lanceolatum*, *circœsans*, *palustre* and others the endosperm is strictly cup-shaped, while in *G. asprellum* and *G. triflorium* the groove is still farther reduced so that in the latter species the endosperm is nearly spherical and solid. In all cases, the space not occupied by the endosperm is at first filled by the seed coat. This at length breaks down in case of the hollow spherical endosperm, leaving an air chamber in the mature fruit.

REPORT UPON THE PROGRESS OF THE BOTANICAL SURVEY OF NEBRASKA. By Prof. CHARLES E. BESSEY, University of Nebraska, Lincoln, Nebraska.

BRIEF history of the survey, followed by an account of the work accomplished and the publications issued.

The survey was formally organized in 1892, by the Botanical Seminar of the University of Nebraska, since which it has brought together a Survey Herbarium of more than 10,000 specimens; published "Reports" I, II, III and IV, including descriptions of new species and lists of additions; published parts I, II and XXI of the "Flora of Nebraska," etc., etc. The total number of species now known to occur in the state is about 3,400.

[This paper will be printed in The Plant World for Jan. or Feb., 1898.]

BACTERIOSIS OF CARNATIONS. By ALBERT F. WOODS, Assistant Chief, Division of Vegetable Physiology and Pathology, U. S. Dept. of Agriculture, Washington, D. C.

A DISEASE of carnation leaves usually mistaken for Bacteriosis was shown to be produced by the puncture of aphides and thrips. The spots on the leaves start as minute clear dots, which may be readily seen when the leaf is held up to the light. The dots increase slowly in size and often run together, the involved cells becoming œdemic. Finally the œdemic

cells collapse and dry out, producing a whitish, sunken spot. In the earlier stages of the trouble these spots do not contain parasitic organisms of any kind, but may become infested by several fungi and bacteria after the death of the cells. All varieties of carnations studied react in this way to the punctures of aphides and thrips, but some varieties more than others.

[This paper was accompanied by specimens and photographs. It is printed in the Botanical Gazette for September, 1897.]

WAKKER'S HYACINTH BACTERIUM. By Dr. ERWIN F. SMITH, Assistant Pathologist, Washington, D. C.

WAKKER'S studies of the bacterial disease of hyacinths were made in the Netherlands during the years 1882 to 1885. He published five papers on this subject,—one in German, three in Dutch, and one in French. With the exception of a purely histological paper by Prillieux, on a supposed bacterial disease of wheat and some preliminary work on pear blight by Burrill, Wakker's first paper (*Botanisches Centralblatt*, 1883) is the earliest publication of any importance on a bacterial plant disease. Dr. Wakker's papers attracted widespread attention both from their apparent thoroughness and because, at that time, it was very generally believed that bacterial diseases were confined to animals.

Since 1885 no botanist or bacteriologist seems to have reëxamined this hyacinth disease until the subject was taken up by the writer in 1896. Diseased bulbs were procured from Holland, and a careful study made to determine how far Wakker's interesting statements would be borne out by another investigator working with modern appliances. This paper gives the result of experiments during the past ten months. In the main, my work confirms that of Wakker. The organism which he isolated is the cause of the disease and the symptoms in my inoculated plants progressed in the identical way described by him. The organism is entirely different from that isolated by Heinz and named *B. hyacinthi septici*, although the two have been confused by various writers. My studies have not only confirmed Wakker's statements respecting the pathogenic nature of the organism and the peculiar symptoms it produces, but have also greatly extended our knowledge of its biology, have brought to light its relationships, and have incidentally led to the discovery of one or two errors in Wakker's description,—errors resulting from the imperfect methods of isolation in vogue at that time. The organism has one polar flagellum and belongs in Doctor Miguls' genus *Pseudomonas*.

ARE THE TREES RECEDING FROM THE NEBRASKA PLAINS? By Prof. CHARLES E. BESSEY, University of Nebraska, Lincoln, Neb.

THERE are sixty-five species of native trees in the state; of these fifty-six have entered from the southeast, while but nine have come down from

the Rocky Mountains. A careful examination of some of these seems to show that they are still advancing, *e. g.*, the bur-oak from the southeast and the pine from the west. Some trees are at a standstill, or nearly so. None are known to be receding.

[This paper was illustrated by diagrams. It will be printed in Garden and Forest, Nov. 17, 1897.]

REPRODUCTIVE ORGANS AND EMBRYOLOGY OF DROSERA. By Prof. C. A. PETERS, Edinboro, Pa.

THESE investigations were carried on in the Botanical Laboratory of the University of Michigan, during the year 1896-7. The object was to gain some knowledge of the embryology of this family of plants and to learn something of the phenomena of nuclear division in this particular species.

The ovules are finally anatropous and are attached along a thick cushion of tissue in the region of the midrib of the carpellary leaves. Each nucellus produces a sporogenous layer of four cells but no tapetum. Three cells of the sporogenous tissue soon disintegrate leaving the fourth which is the mother cell of the embryo sac and which undergoes subsequent cell division as is usual in angiosperms.

The microsporangium produces a tapetum which originates and disappears in a manner strikingly similar to the sporogenous tissue of the ovule. From the first division of the pollen mother cell are produced two cells lying in close proximity which soon divide thus producing the tetrads. The first spindle disappears before the second two are formed. We have here then, in this respect, a process which corresponds to the monocotyledonous type.

In the development of the embryo a suspensor consisting of but two cells is produced. Otherwise the development is in accordance with well known types of other dicotyledons except in a few minor points.

In preparation for cell division in the sporophyte the chromatin does not form a double thread either by the union of two threads or the splitting of one, but accumulates in certain areas into larger granules which finally unite producing sixteen rectangular prismatic chromatin bodies. These take positions in the equatorial plane against the nuclear membrane where the sixteen bodies are united into eight double bodies or chromosomes. In the reduction divisions the chromatin is collected into only half as many bodies in each of the stages corresponding to those of the ordinary division.

THE DEVELOPMENT OF THE SEED OF MELAMPYRUM PRATENSE AND OF CROTON TIGLIUM. By Prof. J. O. SCHLOTTERBECK, University of Michigan, Ann Arbor, Mich.

THE general features of seed development are so well known that a review of them here will be omitted.

The partial parasite *Melampyrum pratense* is in some localities of middle Europe a dangerous pest to the farmer. The seed finds no practical application, but is of interest because it often finds its way into threshed wheat and may, by decomposition of the colorless glucoside rhinanthin which it contains, cause a blue coloration of bread.

The ripe seed is oval in form, about six mm. long and a little curved. At the base of the seed there is a dark brown, conical, spongy appendage which is easily broken from the lighter colored, tough mass of the seed. The whole is covered with a thin, finely striated, papery membrane, which is composed of only one layer of cells.

Nees, the younger, and M. Bentham applied the name strophiole to the conical appendage, while Nobbe called it a caruncle. The appendage, together with the thin enveloping membrane, was termed an arillus by Nees the younger. The raphe of Nees and Endlicher is nothing more than a dark ridge running from the hilum to the micropyle, having no connection whatever with the funiculus of the seed. Finally, the light colored, smooth, tough mass of the seed is called the seed coat, or testa, by Moeller, while he fails to state the origin, or province, of the thin membrane which surrounds it. To clear up these contradictory interpretations was the object of the study of the first mentioned seed.

Alcohol material was found to be practically worthless because of the darkening of the tissues caused by the decomposition of the colorless glucoside before mentioned; therefore, the study was pursued upon fresh growing material.

The flatly ovate ovary, which is about 1.5 mm. long always possesses at its base a rather large, staple-shaped gland, or nectary, which, in a very recent work, has been called a young ovule. The youngest ovules examined, about 200 micromillimeters long, show an anatropous form and only one integument. In a later stage the micropyle appears opposite to where we should naturally expect it. In this stage, too, we see the beginning of a cavity of lysigenous origin containing plasma and nuclei at the base of the embryo sac. This peculiar cavity remains conspicuous throughout the entire development, but becomes almost, if not entirely, obliterated in the mature seed. What its function is to the developing ovule has not been determined.

The nucellus at no time contains more than one layer of cells, and it becomes entirely absorbed shortly after fertilization of the ovule. The embryo sac is at first a mere tube, almost capillary in diameter; but, as development proceeds, there is an enormous production of endosperm with consequent enlargement of the embryo sac. The capillary micropyle, which is almost straight in the youngest stages of the ovule, curves backward in the later stages to such an extent that its orifice is shifted from the apex, or the usual position, to the middle of the funiculus side of the ovule.

The well-developed endosperm soon suffers a constriction near its lower end. The lower smaller portion develops into the brownish, spongy conical appendage, which has been so variously and wrongly designated. The upper larger portion develops into thick-walled, cellular tissue, and

becomes gorged with aleurone and fixed oil. The single integument, which usually never exceeds ten rows of cells in thickness, finally becomes absorbed with the exception of the epidermis, which remains to envelop the kernel of the seed. Attached to the side of the conical appendage is a short, thick funiculus, which is seldom present, however, on the commercial seed. The thin coat, which entirely surrounds the seed, is so easily removed by friction that many seeds are apparently composed of only the naked endosperm; therefore, the true endosperm, which is mainly composed of reserve cellulose, or Moeller's so-called seed coat, acts as the protective organ for the small embryo.

A peculiar feature of the cells of the seed coat is that the substance of their large nuclei is arranged in such a manner as to form very distinct reticula of anastomosing threads.

To sum up, then, we have in this seed of *Melampyrum pratense* not a strophiole, which is an enlargement of the outer integument along the raphe; nor a caruncle, which is an hypertrophy of the outer seed coat at the micropyle; nor an arillus, which is a modified funiculus; but a mass of degenerated endosperm which is loosely attached to the main part of the seed and is easily removed from it.

In certain particulars this seed is very similar to the coffee seed. The thin membrane, or seed coat, corresponds to the membrane which encloses the coffee seed and is folded together in the well-known cleft. The substance of the endosperm is likewise composed of reserve celullose, as is the case in the coffee seed.

CROTON TIGLIUM.

Having come into possession of valuable alcohol material of different stages of the seed of *Croton Tiglium*, its study was taken up to compare its development with that of the seed of its near relative, the castor-oil seed. In morphological characters, the croton-oil seed is very similar to that of the castor-oil seed. Both possess a very hard, thick, protective shell, outside of which there is a thin, more or less soft coat, which has more than once been called an arillode. The caruncle of the castor-oil seed is usually present in the commercial article, while that of the croton-oil seed usually falls off at maturity. Finally, the outer coat of castor-oil seed is mottled with irregular areas dark brown and white in color, while in the croton-oil seed it is spongy and cinnamon brown in color. The ovule of *Croton Tiglium* is like that of *Ricinus communis*, anatropous in form with two integuments, the inner one being twice the thickness of the outer one. The vascular strand of the funiculus, after passing through the chalaza into the inner integument, branches profusely, is quite closely applied to the epidermis of the inner integument and reaches way up to the endostom. The nucellus, which is cylindrical and very long, reaches out of the micropyle, and after suffering a slight bend is partially embraced by the obdurator. This peculiar condition gives at first the impression of a large pollen tube, but its cellular structure precludes the possibility of this interpretation. The embryo sac is globular at its base

and is drawn out to the tip of the nucellus like a capillary tube. A thorough search of the literature on seed development revealed only one other case where the nucellus behaved in a similar manner, and that is in the seed of *Croton flavens*, which was first reported by Keyser. This anomaly reminds one very much of the ovules of *Torenia asiatica*, and *Santalum album*, where the embryo sac alone, and not the nucellus, protrudes from the micropyle.

It is not at all improbable that this unusual growth of the nucellus will be found to pervade the ovules of all plants of the genus *Croton*. At any rate, it would be highly interesting and profitable to compare the development of all its species. As to the reason of this peculiar nucellus growth, I am of the opinion that it is for the purpose of securing for the ovule certain fertilization, and that it is not a useless phenomenon.

After fertilization, the micropyle gradually constricts the nucellus within it so that the portion protruding dies and is thrown off. In the mature seed the nucellus is completely absorbed. With the further development of the ovule we see the inner integument becoming obliterated and absorbed to such an extent that practically nothing remains but the epidermis, which eventually becomes so thick as to produce the hard shell which is found in the ripe seed. The cells of this epidermis of the inner seed coat are composed of thickened, lignified macro-sclereids, which are from fifteen to twenty times as long as they are thick. Inside of this shell-like seed coat there is a thin, almost white, layer of collapsed tissue of the inner integument, which is usually designated as the tegmen. The outer integument remains quite intact throughout the entire development, that is, the cells of its tissue do not suffer any absorption. The tissue between the inner and outer epidermis becomes collapsed however to such an extent, that a cross section shows scarcely more than the tablet-shaped cells of the outer epidermis and the short delicate palisade cells of the inner epidermis.

The cotyledons are very thin, leaf-like in structure, and are imbedded in endosperm rich in aleurone and croton oil. In general the structure of the mature seed is practically the same as that of the castor oil seed. The most important distinguishing feature then of the developing croton-oil seed is the peculiar prolongation of the nucellus mentioned.

Contribution on wild and cultivated roses of Wisconsin and bordering states. By J. H. Schuette, Green Bay, Wis.

The variability of the roses is discouraging and debarring to the study of this "queen of flowers"—even *Rosa Carolina* and *blanda* interchange the usually constant characters of their sepals and pricks. There is no diminution of this difficulty by the reduction of all varieties of roses to one species; separation into good species or distinguishable varieties is the best way of advancing our knowledge of this group.

Valuable characters are furnished by the armature of the plant and the habit of the sepals. An interchange of these for consecutive seasons means at least variations as do several differences in other characters displayed in extensive and distant localities. Accidental causes are usually limited.

On these principles the additional synoptical tables are constructed. Some remarks may be allowed on a few species and some more important new variations.

Rosæ Engelmanni and *Sayi* are closely allied, but the former is distinct by oblong fruits and straight fruit-pedicels. *R. acicularioides:* Leaflet-teeth serrulate; stipules and bracts resinous-glandular beneath, compound-stipitate-glandular on the margins. *R. blanda nuda:* Unarmed; sepals spreading; leaflets short-pointed, rather finely serrate; stipules scarcely enlarged, glandular-toothed as are the lanceolate bracts. *R. blanda glandulosa:* Unarmed; fruits oblong or obovate; sepals glandless; rachis, stipules and bracts glandular. *R. blanda subgeminata:* Prickly to the top; twin-prickles intermittent and irregular; sepals mostly spreading. *R. blanda carpohispida:* Prickles as in last, but twins none; receptacle and pedicels hispid.

R. Carolina sepalorelevata: Sepals erect, persistent; flowers some weeks earlier than those of the type. *R. C. inermis:* Nearly unarmed, often prickly below; sepals mostly spreading. *R. C. aculeata* and *lava* are more strongly armed with spines and prickles, flowering earlier than the type; receptacle and pedicels nearly glandless; the former is erect, short-branched, has dilated stipules; the latter spreading, rather long-branched, stipules long, upturned in the lower, gradually flattened in the upper part.

DEVELOPMENT OF THE POLLEN OF ASCLEPIAS CORNUTI. By FANNY E. LANGDON, Ann Arbor, Mich.

[*Communicated by Prof. V. M. Spalding.*]

THE writer describes in detail the developmental history of the pollinia of *Asclepias Cornuti*, Dec., as studied in serial sections, corrects the account of Corry, the only previous writer, as to the formation of the pollen mother cells and of the outer wall of the pollinia, and discusses the significance of certain histological changes in the tapetum not hitherto observed and which are presumably of physiological importance. The paper forms part of an extended study of the morphology of *Asclepias*.

[This paper was illustrated by charts. It will be printed in The Annals of Botany.]

SOME CHARACTERISTICS OF THE FOOTHILL VEGETATION OF WESTERN NEBRASKA. By Prof. CHARLES E. BESSEY, University of Nebraska, Lincoln, Nebraska.

THE foothill region is an elevated plain twelve hundred metres or more above sea-level upon which occur two uplifts, which run out eastward from the Wyoming mountains; viz., Pine Ridge on the north, fifteen hundred metres, and Cheyenne Ridge on the south, seventeen hundred metres above sea-level. The Box Butte plains are covered with a uniform grass formation, while upon Cheyenne Ridge occur considerable bodies of trees, mostly of pines, with red-cedar, box-elder and others.

[This paper will be printed in the American Naturalist for December or January.]

CELLULOSE-FERMENT. By Prof. FREDERICK C. NEWCOMBE, University of Michigan, Ann Arbor, Mich.

HITHERTO a cellulose-dissolving enzyme has been reported as extracted from only one of the higher plants, namely, from the malt of *Hordeum vulgare*, unless we accept the claim that diastase will dissolve cellulose. The author has succeeded in extracting such a ferment from the cotyledons of seedlings of *Lupinus albus*. The ferment is extracted with water from the minced, dried and ground cotyledons. The watery extract is precipitated with alcohol, and the precipitate collected on filter-paper and dried.

The ferment works well in an acidulated watery solution. Sections of the cotyledons of the seed of the lupin and of the endosperm of the barley, when laid in such a solution, undergo marked changes in a few hours. In twenty-four hours the walls of the barley disappear, the middle lamella of the lupin is dissolved, and the inner lamella of the lupin is somewhat swollen and partially gelatinized. Within a few days, the lupin membranes have become invisible, except for a contour line. The application of chlorzinciodine or Bismarck brown shows, however, that a *skeleton* of the whole inner lamella is left even after two weeks' action of the enzyme. It is possible that the protracted action of the enzyme would cause all the wall to go into solution.

MECHANISM OF ROOT CURVATURE. By J. B. POLLOCK, Ann Arbor, Mich.

[*Communicated by Prof. V. M. Spalding.*]

THE writer discusses the subject historically, giving an account of the theories of DeVries, Sachs, Wortmann, Noll, Kohl and others, which is followed by an extended report of original experiments on the path of

transmission of stimulus, tissue tensions, the action of cortex of stimulated roots when freed from the axial cylinder, the effect of stimulation on tension, mechanical bending, results of plasmolyzing cells of curved roots, migration of protoplasm, and water content of convex and concave sides of curving roots. Theoretical considerations based on experiment lead to the conclusion that no theory hitherto advanced can be accepted as adequate for the explanation of the mechanism of curvature. The writer presents a theory which is believed to be in harmony with the present condition of experimental knowledge, viz.: The impulse produces on the side which becomes concave a change in the protoplasm of the cortical parenchyma which makes it more permeable to water. The elasticity of the cell walls forces water out of the cells into the intercellular spaces. This shortens the cells of that side. At the same time, the stimulus causes an extension of the cells on the side which becomes convex. By the shortening of one side the resistance to curving toward that side is removed, and by the lengthening of the other side the tendency to curve toward the shorter side is increased. The curve then follows from purely mechanical conditions existing in the root.

[This paper will be printed in the Botanical Gazette.]

THE TOXIC ACTION OF PHENOLS ON PLANTS. By Prof. RODNEY H. TRUE and CARL G. HUNKEL B.S., Univ. of Wisconsin, Madison, Wis.

It has been shown by a number of authors, Engelmann, Pfeffer, Nägeli and others, that plants are very sensitive toward the action of many chemical substances, certain classes of compounds in very minute quantities proving strongly poisonous. This sensitiveness of plants to chemical substances has proved a very delicate test for certain compounds diluted far beyond the capacity of the usual means of chemical identification. When solutions of definite content are used this sensitiveness of plants admirably fits them to serve as test objects for the investigation of many chemico-biological questions, especially those connected with the action of poisons. A study of the toxic action of the group of substances known as phenols on the radicles of *Lupinus albus* L., and on *Spirogyra* sp. has shown that living organisms respond in a definite manner to substances having a definite constitution; the reaction of the protoplasm being therefore governed by chemical laws.

Electrolytic dissociation of the molecules of these compounds into ions plays a subordinate rôle in their physiological action, the undissociated molecules, therefore, determining to a large degree the physiological properties of the substances. As instances of substances in which electrolytic dissociation plays a more or less pronounced part, picric acid and salicylic acid may be noted. In the cresols and the mono-nitrophenols a considerable part of the toxic activity of the solutions seems to be due to

the products of such dissociation. In pyrogallol and methyl salicylate other processes of decomposition produce substances which dissociate electrically and increase the poisonous activity of solutions of these substances. Some phenols are comparatively weak in their integrity but quickly break down to much more vicious substances. Pyrogallol, pyrocatschin and hydroquinone are examples.

Certain radicles seem to have specific properties when introduced into the molecule, modifying the toxic value of the same. The number of hydroxyl (OH) groups present in the molecule seems in itself to have little influence on the toxic action of the phenol. The introduction of CH_3 groups into the benzene nucleus increases the toxicity to a considerable but somewhat variable degree. The introduction of the isopropyl group (C_3H_7) into the cresoles increases the toxic value of these substances. The presence of one or more nitro (NO_2) groups increases the toxic action to a great degree. An increase in the number of (NO_2) groups does not seem to increase this toxic value. When the hydrogen atom of an OH group is replaced by a CH_3 group little influence on the toxic action toward these plants is exerted. The carboxyl group (COOH) brings an added degree of toxicity corresponding to the degree of dissociation.

IS THE CHARACTERISTIC ACRIDITY OF CERTAIN SPECIES OF THE ARUM FAMILY A MECHANICAL OR PHYSIOLOGICAL PROPERTY OR EFFECT? By CHAS. PORTER HART, M.D., Wyoming, Ohio.

ACCORDING to Professor Lazenby,[1] "the raphides, or needle-shaped crystals," found in the interior of the cells of certain species of the Arum family. "appear to have the power of producing the intense acridity characteristic" of the internal use of the juice of these plants on the human system. This statement, and the conclusions accompanying it, are in such marked variance with the results of the writer's investigations, that a brief *résumé* of the latter's observations and experience may be of sufficient scientific interest to warrant comparison.

In the fall of 1875, I made a careful and extensive experiment or "proving," of the juice of *Arum dracunculus* (*A. dracontium*, Raf.), or dragon root, on myself, which proving was published in the American Observer for November of that year. This article was afterwards re-edited, with notes, by Professor Hale of Chicago, and published in circular form. In the latter a comparison is made with the physiological effects produced on the human system by other members of the Arum family, especially the *A. triphyllum*, *A. maculatum*, and *A. Italicum*.

As bearing directly on the questions at issue, the author here makes a few excerpts from his paper just referred to, which consists of notes jotted down at the times the experiments and observations were made.

[1] See Abstract of Prof. L.'s paper in the Proceedings A. A. A. S., Vol. XXXIX, p. 283.

[Here follow the extracts referred to. See American Observer, Nov., 1875.]

These experiments, together with the experiences and observations of others, have led the writer to the following conclusions:

1°. The sensation produced on the human system is both local and general.

2°. The local sensation is, for the most part, similar to the general sensation, being of that peculiar prickling or tingling character known as formication.

3°. This tingling sensation is not confined to any particular part of the body, either external or internal, cutaneous, mucous, or visceral, but sometimes affects two or more of these tissues simultaneously.

4°. This symptom is a purely physiological effect of the active principle of the plant, transmitted primarily through the vascular system to the sensitive branches of the cerebro-spinal system of nerves, affecting more particularly the parts supplied by their terminal branches, the vasomotor system, the cutaneous surfaces, etc.

5°. The effect is produced by a highly volatile principle, as is conclusively shown by the fact that it is entirely lost by atmospheric exposure, as in the process of drying.

6°. The fact that dialysis destroys the acridity is in perfect harmony with these facts.

7°. That the sensation is in proportion to the number of inorganic crystals contained in the cells, as asserted by the authority I have quoted, is, to say the least, highly problematical.

8°. *If* "filtration separates the acridity from the clear juice," which I am not prepared either to concede or deny, I should be inclined, in view of the above facts, to refer the phenomenon, not to its separation by filtration, but to its escape during the filtering process in consequence of the extreme volatility of the active principle.

SOME REASONS FOR PLANT MIGRATION AND THE WAY PLANTS FLEE FROM THEIR ENEMIES. By Prof. W. J. BEAL, Agricultural College, Mich.

IN managing a small botanic garden for twenty years, experiments and observations have been made in this line of work. For a few years plants set in the garden usually thrive, but many of them sooner or later need a change. The ground becomes clogged with root-stocks, or the soil depleted, or some one or more kinds of insects or fungi settle down there and in various ways the plants are annoyed.

In case of a number of kinds of mints, sunflowers and others the central part of a patch of each species exhibits signs of failing or dies completely. Numerous other examples were given, all tending to show that plants shift from place to place not merely to extend and multiply the species and reach a fertile soil, but to enable them to flee from the great

numbers of their own kind, and their enemies among animals and parasitic plants, and especially that they may come in contact with other plants of the same species to be improved by cross-fertilization. The adventurers among plants often meet with the best success, not because the seeds are larger or stronger or better, but because they find for a time more congenial surroundings. Every horticulturist knows that apples grown in a new country are healthy and fair, but the scab and codling moth and bitter rot and bark louse sooner or later arrive, each to begin its particular mode of warfare. Peach trees in new places remote from others are often easily grown and free from dangers, but soon will arrive the yellows, borers, leaf-curl, rot and a host of other enemies to combat. I have often noticed that while pearblight decimated or swept off large portions of a pear orchard, a few isolated trees scattered about the neighborhood, usually remain healthy.

Perhaps one reason why some plants have become extinct or nearly so is their lack of means of migration.

As animals starve out in certain seasons when food is scarce, or, more likely migrate to regions which can afford food, so plants desert worn-out land and seek fresh fields. As animals retreat to secluded and isolated spots to escape their enemies, so many plants accomplish the same thing by finding the best places with some of their seeds sown in many regions. Frequent rotations seem to be the rule for many plants when left to themselves in a state of nature. Confining to a permanent spot invites parasites and other enemies and a depleted soil, while health and vigor are secured by frequent migrations.

[This paper will be printed in The Plant World, beginning Vol. 1, p. 27.]

STOMATA ON THE BUD-SCALES OF ABIES PECTINATA. By ALEXANDER P. ANDERSON, Clemson College, South Carolina.

IN the Metaspermæ, stomata occur, not only on all leaves, but are usually found on all well developed bud-scales. The leaves of Gymnosperms have fewer stomata, hence one would expect to find fewer or no stomata on their bud-scales. This has been found to be true and it has always been thought that stomata never occur on the bud scales of Gymnosperms.

Grüss in his "Beiträge zur Biologie der Knospe" (Jahrb. für Wissenschaftl. Botanik 23 : 642) says that stomata are never present. Schumann (Bibliotheca Botanica, 1889, Heft 15, s. 3) makes a similar statement.

In a recent paper (Sonderabdruck aus der Forstl. naturw. Zeitsch. 1896, s. 14), the writer called attention to the occurrence of stomata on the bud-scales of *Abies pectinata*.

The stomata are found only near the base and on the lower (dorsal) surface of the scales. They are not arranged in rows but are scattered and irregularly distributed, being found even on the midrib where they never occur on the leaves.

[This paper will be printed in the Botanical Gazette.]

COMPARATIVE ANATOMY OF THE NORMAL AND DISEASED ORGANS OF ABIES BALSAMEA (L.) MILLER, AFFECTED WITH ÆCIDIUM ELATINUM, ALB ET SCHWEIN. By ALEXANDER P. ANDERSON, Clemson College, South Carolina.

THE anatomy of different species of Abies varies slightly under normal conditions. This is especially true of the leaves and younger branches that in some species have tissues and structures which are rudimentary or wanting in others. Such structural and anatomical diversity either become more marked or are entirely lost in hypertrophied or atrophied organs.

The leaves on the lateral branches of *Abies balsamea* are always twisted at the base, giving them the distichous direction peculiar to species of Abies. The leaves of the erect shoots are not twisted nor transversely heliotropic; they are shorter and thicker than those of the lateral branches. Cross sections show also the greater development of certain tissues especially the hypodermal strengthening cells and transfusion tissue of the pericycle.

The anatomy of the diseased leaves agrees in many respects with that of the normal leaves of the erect shoots.

In their winter condition buds of *Abies balsamea* are always covered with a layer of resin, one to three millimeters thick. This large amount of resin exudation is due to the fact that the bud-scales have from two to six resin-canals and are fringed with large numbers of thin-walled marginal hairs through which the resin diffuses. The epidermis of the exposed scales is sclerotic and thick-walled allowing no resin to diffuse through it.

Resin canals are found only in the primary cortex. They often remain functional in the bark of trees fifty to eighty years old. Resin vesicles or blisters are formed in the primary cortex by the radial and tangential division of the thin-walled epithelial cells of the resin-canals.

[This paper will be printed in the Botanical Gazette.]

ON A NEW AND IMPROVED SELF-REGISTERING BALANCE. By ALEXANDER P. ANDERSON, Clemson College, S. C.

IN the course of some experiments on the growth of cucurbitaceous and other fruits it was found that the fruits were extremely sensitive to the temperature and moisture variations of the atmosphere. With a small degree of the relative humidity, the fruits, after having reached their maximum in growth, would begin to decrease in weight during the day. It was found that, in order to record the actual variations in the weight of the fruit, an instrument that would record automatically the increase as well as the decrease, would have to be devised.

After repeated trials and alterations, the writer has succeeded in devising a self-recording balance which will record automatically any increase or decrease in any object placed on the balance. The instrument is an improvement on the one previously described in the Minnesota Botanical Studies (Part 4, 177, 1894).

[A description with drawings of all the parts of this instrument will be published in the Botanical Gazette.]

THE CORRELATION OF GROWTH UNDER THE INFLUENCE OF INJURIES. By Dr. C. O. TOWNSEND, Columbia University, New York, N. Y.

THE purpose of the experiments which form the basis of this paper was to determine in what time, through what distance, and to what extent, an injury inflicted upon one part of a plant will influence the growth of the injured and of the uninjured parts. The experiments were carried on in the Botanical Institute at Leipsic, under the direction of Professor Dr. Pfeffer.

The external causes of the variation of the rate of growth were eliminated as far as possible, in order to study more accurately the influence of the injuries. Hence the plants were grown in damp chambers, in diffused daylight and in all possible cases at constant temperature. Most of the experiments were performed with seedlings, for which purpose *Phaseolus multiflorus*, *Vicia Faba*, *Lupinus alba*, *Helianthus annuus*, *Cucurbita pepo*, *Zea mais*, *Avena sativa*. *Hordeum vulgare* and *Secale cereale* were used.

In the experiments with older plants Calla and cuttings of Salix were used in addition to the material used with seedlings. A number of experiments were also performed with *Phycomyces nitens*.

In experimenting with seedlings, either the plumules, shoots, leaves or roots were removed as a whole or in part, or the shoots, leaves or roots were split near the base or near the tip for a distance of from five to ten millimeters. If the injury was very slight no appreciable change in the rate of growth of either the injured or of the uninjured part was apparent. A slightly stronger injury produced an acceleration in the rate of growth of the uninjured part. The acceleration became apparent in from six to twenty-four hours and often continued for several days, being followed by a return to the normal rate of growth. If the injury was still more severe the growth of the uninjured parts was retarded. The retardation became noticeable in from six to twenty-four hours, continued from one to several days and was followed in some instances by a period of acceleration.

The older plants were treated in practically the same manner as the seedlings with substantially the same results. The changes in the rate of

growth often took place earlier and were more marked than in the case of seedlings.

In the experiments with Phycomyces a mycelium was cut or one of two or more sporangium stalks was removed. The result was a sudden and marked retardation in the rate of growth of the remaining sporangium stalks. In no case examined, however, did the growth entirely cease for any appreciable length of time, and the normal rate of growth was recovered in from thirty to sixty minutes.

The conclusions reached in regard to the higher plants are, first, that the change in the rate of growth due to injury takes place gradually, reaches a maximum and gradually returns to the normal rate; second, that the distance through which the influence of the injury was found to exert itself varies from 0 to several hundred millimeters depending upon the degree of injury; third, that the total variation in growth due to injury is from 0 to 80% of the normal growth depending upon the degree of injury and upon the nature and conditions of the plants under examination.

[Diagrams were used to illustrate the variations in growth. This paper will be printed in Annals of Botany, Dec. 1897.]

THE BOTANICAL COLLECTIONS OF THE CORNELL ARCTIC EXPEDITION OF 1896. By W. W. ROWLEE and K. M. WIEGAND, Cornell University, Ithaca, N. Y.

A PARTY of geologists from Cornell University accompanied Lieutenant Peary last summer to the shores of Labrador and north Greenland. In addition to the geological material collected, extensive collections were also made of the plants in the regions visited. These were nearly all obtained at three points, viz., Turnavik Island, Labrador, Hudson Strait and White St. Baffins Land, and the coast of Greenland from Disco Island to the Nugsuak Peninsula in Lat. 74° 15′. At the latter place an island seven miles inland from the ice front was visited, upon which the following nine species of plants were found to have already gained a foothold: *Savastana alpina*, *Carex Bigelovii*, *Juncoides hyperboreum*, *Papaver alpinum*, *Cardamine bellidifolia*, *Potentilla emarginata*, *Cassiope tetragona*, *Vaccinium uliginosum microphyllum*, *Antennaria alpina*. In all about one hundred and thirty species and varieties were collected. Many of these were rare and a few added new localities to those already known. No new species were found although some specimens of *Carex* and one or two forms of *Salix* were quite different from any already described. More material of these forms should be collected before any discussion is made regarding them. The sets of *Salix*, *Gramineæ* and *Cyperaceæ* were unusually complete, and in all formed about one-fourth of the entire collection.

On the nature of certain pigments, produced by fungi and bacteria with special reference to that produced by Bacillus solanacearum. By Dr. Erwin F. Smith, Assistant Pathologist, Washington, D. C.

The paper might perhaps better have borne the title, "Speculations on the Origin of Humus compounds." The dark brown pigment produced by the potato-rot bacillus, *Bacillus solanacearum* (see Bulletin 12, Div. Veg. Phys. and Path., U. S. Dept. Agric., 1896), will not dialize readily and is precipitated by compounds of calcium and of iron. It is suggested as a working hypothesis that the humus compounds of the soil are largely if not exclusively due to the action of fungi and bacteria on the carbohydrate materials of animals and plants, especially of the latter.

Description of Bacillus phaseoli n. sp., with some remarks on related species. By Dr. Erwin F. Smith, Assistant Pathologist, Washington, D. C.

Bacillus phaseoli n. sp. is a short rod with rounded ends, yellow on various media, motile in early stages of growth, decidedly pathogenic to beans and some related legumes, and closely related to *Pseudomonas hyacinthi* (Wakker) and *Pseudomonas campestris* (Pammel). Its thermal death-point is approximately 49° C. and it will not grow in the closed end of fermentation-tubes in beef broth or peptone water with any of the ordinary sugars. It exerts a powerful diastatic action on potato starch. On bean pods it causes watersoaked spreading spots It has been under observation in pure cultures for about thirteen months and there is no doubt whatever as to its parasitic nature, all of Koch's canons for determining this point having been fully complied with. The three organisms here mentioned were compared and contrasted and cultures of each exhibited. Some of the relationships of these three bacteria are exhibited in the following tables.

(1) SOME POINTS OF AGREEMENT.

SPECIES.	COLOR IN MASS.	MORPHOLOGY.		BEHAVIOR ON GELATIN.	RELATION TO AIR.	FERMENTING POWERS.			GROWTH AT		CYTOHYDROLITIC ACTION IN HOST PLANTS.
		SHAPE.	FLAGELLA.			Gas.	Acids.	Behavior in fermentation tubes	37°	40°	
B. phaseoli n. sp.	yellow.	Short rod with rounded ends.	? (Motile.) All attempts to stain have failed.	Slow liquefaction. Finally all peptonized.	So far as tested, strictly aerobic.	None.	None.	Will not grow in closed end with any of the sugars.	Feeble	None.	Moderate. More decided on potato. Dissolves middle lamella.
Ps. campestris (Pammel).	"	"	One polar flagellum.	"	"	"	"	"	"	"	Moderate.
Ps. hyacinthi (Wakker).	"	"	"	"	"	"	"	"	Doubtful if any. Grows at 34° C.	"	"

(2) SOME MEANS OF DISTINGUISHING.

SPECIES.	GROWTH ON STEAMED POTATO.	DIASTATIC ACTION ON POTATO STARCH.	PIGMENT STAINING SUBSTRATUM.	THERMAL DEATH POINT. (10 M. EXPOSURE.)	PATHOGENIC TO	NOT PATHOGENIC TO
B. phaseoli n. sp.	Copious (at 20° to 30° C.)	Powerful	?	49° C	Beans, Lupins, Peas (?)	Cabbages, hyacinths, or Pelargoniums.
Ps. campestris (Pam.)	"	Moderately strong.	Dark brown stain on cooked turnips and in host plants.	51° C.	Cabbages, turnips, mustard, rape, etc.	Beans or hyacinths.
Ps. hyacinthi (Wakk.)	Rather meager.	Feeble	Feeble brown in host and on turnip.	47° C.	Hyacinths.	Cabbages or Beans.

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ADDRESS

BY

W J McGEE,

VICE PRESIDENT, AND CHAIRMAN OF SECTION H.

THE SCIENCE OF HUMANITY.

I.

HUMANITY is a favorite theme of poet and philosopher, novelist and historian, dramatist and moralist. The changes rung on the theme run the entire gamut of feeling and thinking; its burden is caught in song and story and crystallized in books; and no sweeter strains have ever been sung, no grander scenes enacted, no nobler lines penned, than those fertilized by the touch of human (and solely human) nature that makes the whole world kin.

The chief subject of thought among all races is humanity in some of its numberless aspects; the chief part of the literature of civilized nations relates to humanity; the chief activities of all men are inspired by humanity. Yet — and this is a modern marvel — for the greater part the thought is vague, the literature random, the activity unorganized; *i. e.*, this most important of all subjects-matter and objects-matter in human ken has hardly been brought into the domain of that definite knowledge called Science. It is meet to inquire why this is so; and, to the end that the inquiry may be answered clearly, it is needful first to define humanity and then to consider what knowledge is and the way in which science has come to be; later, the half-formed science of that which is proper to intellectual man and most important to his kind may be outlined.

II.

According to the lexicographer, humanity denotes (1) the condition or quality of being human, (2) the character of being humane, (3) the character of being well-bred, (4) mankind collectively, and (5) secular learning or literature.[1] The fourth of these definitions connotes Man — the genus Homo, object-matter of the broad science of Anthropology — viewed in a distinct way, *i. e.*, as a mass or composite body rather than discrete individuals. The fifth definition connotes but a limited field in a vast domain, and is scholastic if not archaic; with this sense the term is chiefly used in opposition to divinity, often in the plural form (though there is good precedent for the use of this plural form in a more general and at the same time a more definite sense).[2] The first three definitions connote a wide range of attributes of Man which, albeit well recognized by all intelligent people, are rarely reckoned among the objects-matter of anthropology, seldom included within the pale of science; yet it is these attributes that especially distinguish Man and set him apart from the mineral, vegetal, and animal worlds, and exalt him above the rocks and plants and beasts of simple nature.

Although commonplace, these definitions are worthy of careful consideration in that they summarize the substance of intelligent thought since the beginning of writing – indeed since its own beginning in the remote unwritten past,— and particularly during the era of unprecedented intellectual activity and scientific progress dating from the issue of Bacon's *Novum Organum;* they carry the wisdom of the ages, and especially of these later days during which wisdom prevails as never before. Viewed separately or in connection with contemporary definitions relating to mankind, they indicate general (although vague) recognition of certain specific attributes of Man, not as an animal but as an ill-defined something known as a Human Being. When the history of thought condensed into the set phrases of the lexicographer is scanned, it is found that bitter controversy has been engendered by the diverse aspects of Man as seen from opposite sides; the disputants, like the storied knights of old, have admired the object, one as silver and the other as

[1] Condensed and rearranged from the "Standard" and "Century" dictionaries.
[2] *e. g.*, in "The Humanities," by J. W. Powell; Science, New Series, vol. I, 1895, pp. 15–18.

gold, and have done doughty battle in defence of their one-sided vision — the biologist, with eyes trained by observation and reason sharpened by long study of living things, sees the silvern side and sounds trumpet for man as an animal, while the litterateur and statesman and philanthropist are half dazzled by the golden glory of Man as a thing supernal. The fair conclusion is that both are right as to what they see and both wrong as to what they fail to see; and in the light of this conclusion it is clear — if the general judgment of the body of thinkers is worth anything — that man has an animal basis on which a noble superstructure is borne. The definitions of the lexicographer, who voices the thought of the world, show that among general thinkers the idea of humanity prevails over the idea of animality, while the current literature of science indicates that the idea of animality is dominant in scientific circles — indeed, some writers on anthropology, the Science of Man, restrict the term to knowledge of the mammalian order Bimana, a limitation excluding the essential characteristic of Man as a thoughtful and emotional being and as an integral part of a collective and interdependent assemblage. Any attempt to harmonize these opposing ideas must begin with definite statement of the meaning attached to the essential term by more catholic anthropologists. So Humanity may be defined, by exclusion, as the condition or quality or character of possessing attributes distinct from those of animals, vegetals, and minerals; or, by inclusion, as (1) attributes or characteristics confined to human beings, comprising (*a*) the condition or quality of being human, *i. e.*, of acting, feeling, and thinking after the manner of human beings, (*b*) the character of being humane, and (*c*) the character of being well-bred; (2) mankind collectively; (3) secular learning and literature.

The supreme importance of humanity as thus defined is indicated by the fact that it is the foremost subject-matter of thought and speech and literature among all peoples, its prominence increasing from savagery through barbarism and civilization and culminating in enlightenment. The essential distinctness of humanity as thus defined appears when its serial relations to the other primary objects-matter of knowledge are considered: just as living things rise above the mineral world by the possession of vitality, and just as animals rise above plants by the possession of motility, so do human beings rise above all other things by the possession of specific attributes rooting in mentality and maturing in the complex

activities of collective life; or just as inorganic matter is the basis for the essentially distinct organic existence, so organic matter and processes form the basis for the essentially distinct superorganic activities of human existence. The importance and distinctness of humanity are indeed such that it behooves naturalists to recognize a fourth realm or world — to extend science from the great realms of the mineral, the vegetal, and the animal into the incomparably broader and richer realm of the purely human; and this extension is the chief end of modern anthropology.

III.

Human Knowledge is constantly increasing. The body or aggregate of Knowledge is imponderable, and may not be counted or measured or weighed; yet it is an entity of prime importance and of universal recognition. Itself indefinite and varying from mind to mind, the sum of knowledge may be divided, albeit roughly, and analyzed, albeit crudely, and the days and years and centuries of its progress among men and peoples may be so studied that its tendencies and perhaps even the laws of its growth may be followed, albeit slowly and uncertainly. Although so indefinite, it is well worth while to try, and try again and still again, to analyze Knowledge and trace its progress; for Knowledge is the end and aim of intelligence, and human progress is measured not more by increase in knowledge of things than by increase in knowledge of Knowledge.

Many students have found it convenient to divide or classify knowledge as individual and common, general and special, empiric and scientific, deductive and inductive, etc., according to the point of view; and these divisions are of use in that they represent first steps in analysis — though it is to be remembered that they are more or less vague or arbitrary, one or both. It may not be bootless slightly to extend this provisional analysis in order to trace more clearly the lines and stages in the growth of intellectual product.

For the sake of gaining clear ideas of relation, it is sometimes useful to project perception by the aid of mental imagery, and thereby to visualize the invisible in the eye of the mind. So the great aggregate of Knowledge is often likened unto a numerical sum, or a reservoir or river fed by many affluents; but a better figure may be found in scientific ideation, and the imponderable

body may be pictured as an indefinite nebula or plasma, constantly growing by accretion and constantly undergoing internal change. This plasma may best be portrayed as for the most part unorganized, with partially or completely organized nuclei and nodes and processes here and there; and there is a certain fitness in conceiving the organized tracts as near the surface where the interactions between external and internal are direct and continuous. In this way the intellectual product of the world may be likened unto a nebula, a cloud gathering in a supersaturated solution, an ameba, or a brain — it may be viewed as a chaos more or less advanced on the way toward cosmos. The image is ideal; it serves merely as an aid in grasping and formulating widespread notions concerning knowledge as an elusive and intangible yet vigorously real and important Something; but it is not essential to correct understanding of the main facts in the growth of Knowledge.

Knowledge is born of the individual brain fertilized by indirect contact with other brains, and is given unto others with a degree of freedom varying with the disposition of the individual and the perfection of his mechanism for conveying thought — gesture, picture, speech, writing, printing; the growth of Knowledge keeps even pace with the acquisition of structures and devices for its expression, and it is a pleasant and significant fact that in general the disposition to dispense Knowledge grows strong and active just as the dispensing mechanism improves, though usually lagging a little behind — much as the verdure follows the vernal shower. So the stage of individual knowledge is initial, the stage of common knowledge consequent; so also individual knowledge is barren and unproductive until turned into the general fund to increase and multiply an hundred-fold; and so too there is progressive growth from the initial stage of individual discovery or invention, through many ill-defined yet successively higher and higher steps, well toward the mature stage of general possession. It is needful to observe that the body of general knowledge can never quite equal the aggregate knowledge possessed by individuals; although stimulated by others, each active individual knows something more than he is able to tell, be he never so free in disposition and facile in expression; and it is the never-ending process of coining and issuing and exchanging the precious product of the cerebral crucible that gives rise to intellectual property-right, and at the same time enriches the great plasma of Knowledge and maintains the

activity essential to its existence. It follows — and this scientific certitude may be commended to a certain class of socialistic schemers — that the relation between individual knowledge and general knowledge is asymptotic, in that although the latter constantly approaches it never can reach the former; indeed, if general knowledge were ever to overtake individual knowledge, through suspension of the laws of intellectuality (undoubtedly immutable as those of vitality), the special province of mental activity would be annihilated and the body of Knowledge would sink into quiescence — and, in the intellectual as in the vital, quiescence is death.

As Knowledge is produced and given unto others the freedom of giving is governed by numberless conditions, including the perfection or imperfection of the mechanism for expression, the avidity or indifference of the chosen beneficiaries, and the price fixed by custom; and so it happens that certain discoveries and inventions are directly communicated only to limited groups of individuals, who thereby accumulate special knowledge. In this way cliques and trades and guilds arise and the germ of caste is planted; in this way, too, specialties grow up through the indifference of the masses and their inability to keep pace with the investigator whose energies are directed along a single line; and, eventually, among the most enlightened peoples, special societies are formed for the purpose of fostering or diffusing discovery and invention and thereby rounding out the rich plasma of human Knowledge. It may be noted that special knowledge is nearly as barren and unproductive as individual knowledge, and is soon blasted by the poison of its own egoism, unless the richer part of its substance is guided toward the general mass — to do work as it advances; for it is by no means to be forgotten that the activity of the great body of Knowledge culminates in the province or zone of special knowledge, and that herein lies the leaven that leavens the whole.

During recent centuries and especially during recent decades specialists engaged in creating knowledge have studied Knowledge itself in the hope of learning its nature and origin; and most of these students have become convinced that the basis of real knowledge is found in the facts of the cosmos as revealed by observation or established by experimentation. So the acquisition of Knowledge begins with noting particular facts and advances to assembling or grouping these facts, *i. e.*, proceeds from observation to generalization; the second process involves the elimination of

the unlike or incongruous, and this leads to discrimination and to the recognition of analogies. In general terms and somewhat provisionally it may be said that the analogies so recognized constitute laws of occurrence, which may themselves be generalized, and that the requisite discrimination of analogies leads to the recognition of homologies, or laws of occurrence and sequence combined; the framing of analogies and homologies being legitimate inference, which develops in hypothesis and matures in theory or doctrine to be finally formulated in laws or principles. Knowledge produced in this regular and simple manner is commonly called inductive, though there is always a deductive element coming over from that general intellectual possession by which even the closest specialist is guided in greater or less measure. Now it is to be noted that the acquisition of Knowledge is largely spontaneous and unconscious — that apperception lags far behind perception, and that only the adolescent and mature among men and peoples are clearly conscious of their own mental processes, or, indeed, of the existence of mental process; it follows that most of the processes just outlined are ill-recognized or not recognized at all, even by the very makers of Knowledge. Moreover the later steps in intellectual acquisition are commonly the first to be consciously noted, so that the majority of men, even unto the present day, have failed to recognize the true source of real knowledge, and have appealed to all manner of mysterious and extravagant sources for part or all of the intellectual wealth of the world; for while the more complex processes alone were recognized inference was exalted and observation was contemned, subtle imagining ran riot and overshadowed sober reason, and scholastic learning — which the practical makers of progress fortunately ignored or repudiated — grew into a labyrinth of deductions from vain postulates and hazy lucubrations. A new epoch dawned when Bacon formulated the inductive method — though he knew not that the method was old as the human mind and that he but recognized that which all men do, whether consciously or unconsciously. Reviewing the course of intellectual acquisition from observation through generalization and inference and theory unto laws of occurrence and sequence, Knowledge may be classified by degree of development, and the simpler and more primitive (whether burdened by assumption or not) may be called empiric, while the more definitely organized product of special study may be called scientific; and, remembering that the proc-

esses of acquiring Knowledge are partly unconscious, that portion which is organized unconsciously may be classed as common sense, or sagacity, or the wisdom of experience, while the consciously organized portion may be called science. This summary of the mode of organizing Knowledge may be trite, yet it serves to show that the methods of the student of Humanity are in no wise different from those pursued in the physical and natural sciences.

In brief, Knowledge is ever passing from the individual to the common and from the special to the general, and thereby its quantity is constantly increased and its utility extended; during recent times it is passing also from the empiric to the scientific, and thereby its quality is improved and its beneficence multiplied.

IV.

When the history of the class of knowledge called Science is scanned, certain tendencies or directions of growth are perceived; and scrutiny shows that these tendencies are in harmony with the course of development of universal knowledge.

1. In general, observation and research begin with the rare or remote and proceed toward the common and the near. This tendency is revealed when the several branches of science are compared. Perhaps the oldest science is Mathematics, which began before history so that its origin is obscure and cannot certainly be traced to definite objective basis; but the nearly contemporary and closely related science of Astronomy rested on observation of the celestial bodies — though the observation was long clouded by the mysticism of astrology. Then as wits were sharpened by mathematical research and astronomical observation, exact knowledge was gradually brought down to nearer bodies and under the guidance of every-day observation; and thus the science of Physics arose so gradually and inconspicuously that its early history is lost. Later shrewd hermits and beldams wrought magic by means of rare substances, and alchemy grew up; and as time passed the manipulations were extended to common things and the ban of secrecy was gradually broken, and so Chemistry arose. These four branches of knowledge concerning the inorganic interacted with mutual benefit, and for several centuries constituted Science, in contradistinction from the vast body of vague thought comprised in

scholasticism and folklore and from the more useful body of commonplace knowledge not yet consciously organized. Still later, attention was attracted by things nearer to mankind in place and character, and first plants and afterward animals were studied systematically, and Botany and Zoölogy arose; but for a long time the most attractive organisms were the unusual and therefore striking, or the specimens brought by travellers from distant lands — indeed, even during the present half century, scientific museum administrators are embarrassed by the tendency of the collector to neglect the common and collect the unusual in his own locality, and it is only within a generation or two that the ordinary plants and animals supplying mankind with food and clothing and other everyday commodities have been subjected to scientific research. In like manner the science of Geology began, soon after botany and zoölogy, with the study of rare minerals and the ancient rocks of remote mountains; gradually the research extended to the nearer hills and valleys and the later formations; and it is only within the present generation that the soil-making deposits on which human life so largely depends have been brought under scientific examination. Last of all the scientific research beginning with the stars and passing to minerals and plants and animals, and through the soil on which plants and animals live, reached Man himself; yet the studied observation began not so much with fellow-citizens or fellow-subjects bound to the observer by ties of consanguinity and affinity as with the abject savage or half-clad barbarian of distant lands; and even to-day, and in the most enlightened nations of the earth, the pictures brought up in most minds by the term Anthropology are those of alien and inferior peoples, or of human curiosities and monstrosities exhibited in midway plaisances if not in circuses and dime museums. Even in scientific circles — yea, among those ranked as anthropologists — there are many who habitually restrict the term to the purely animal side of man, and ignore that broader and nobler side which distinguishes mankind from all other things. So, whether Science be viewed in general or in detail, it is found that its progress is toward the ego — toward the everyday and commonplace perhaps, yet ever toward the more important because the nearer, the more useful because the commoner; and the more nearly it approaches the more clearly it is seen that Science dignifies both student and object of study — that exact knowledge, with Midas touch, turns dross to gold.

2. In general, research begins with the abnormal and proceeds toward the normal. Judging from the habits of present-day barbarians among whom the tempest is studied and the zephyr ignored, the comet remembered and the planet forgotten, the pre-Chaldean astronomers based their first celestial observations on the erratic wanderers rather than the orderly travelers of the sky; and in all ages prodigies — the bizarre and ill formed, the gigantic and dwarfish — have been the first to catch and the longest to hold attention, among casual observers and specialists alike. This tendency toward noting the abnormal, like that of regarding the rare rather than the common, is the easily besetting sin of the touring naturalist and local museum collector, the joy of the unscientific and the despair of the scientific among museum administrators. Clearly seen in geology and zoölogy and botany as the vestige of a primitive past, this tendency to perceive only the abnormal is still strong — indeed almost dominant — in the younger science of anthropology; to-day distorted or wounded or cachectic skulls from the ancient ossuaries of Africa or the huacals of Peru are esteemed far above normal crania of a normal people who have by normal activities aided in making civilization and ennobling the world; to-day the platycnemic tibia and perforate humerus of questionable significance are exalted above the normal members occupied in the march of progress and the conquest of lower nature; to-day there are a flourishing sub-science called criminology and a fantastic fad of extolling and magnifying degeneracy, while the upright in mind and the sound in body are relatively neglected — yet this apparently morbid taste but reflects a tendency of the human mind, and is the promise of better things when the intellect awakened by the abnormal acquires the power of appreciating the normal. Unremembered millenniums of mystical shamanism were required to produce pathology and therapeutics, and centuries of pathology were needed to produce sound physiology and etiology; and in like manner there were generations of mystical and irrational psychomancy before students were able to recognize a basis for the modern and most promising science of psychology. It smacks of the paradox to say that the beginning with the abnormal is the normal course in the making of science; yet the history of each and all of the sciences shows that observation on the abnormal has always led attention to the normal, just as observation on the remote has ever guided attention toward the near; and it is but nat-

ural that the youngest of the sciences should yet retain vestiges of undue magnification of the abnormal.

3. In general, scientific determination proceeds from the qualitative to the quantitative. This tendency is displayed by every branch of science, and so conspicuously that it may be deemed characteristic. It is in accordance with this tendency that estimate precedes weighing and reconnaissance goes before surveying; and it is under the same tendency that scientific progress involves constantly-increasing refinement in observation and ever-growing accuracy in definition.

4. In general, scientific interpretation proceeds from the formal to the physical,[1] from the material or the inert or the static to the dynamic. The positions and movements of the moon and planets were determined with fair accuracy before Newton discovered that the paths of these and all other celestial bodies are fixed by gravity; when this discovery afforded the means for determining position and movement with incomparably greater accuracy. The physical and chemical effects of heat were recognized for generations, and were ascribed to the hypothetic element phlogiston or the imaginary fluid caloric long before Joule and others found it to be merely a manifestation of molecular motion; whereupon physics and chemistry were revolutionized and the forces of nature were gradually harnessed many times more effectively than before.

The ancients recognized vitality and ascribed it usually to a material Something joined to the matter of the body. Some twenty-four centuries ago sagacious Heraclitus conceived life as a universal fire, and less than five centuries ago Paracelsus and after him van Helmont wrote of the *anima mundi* or *archeus*, having in mind a vaguely imponderable thing akin to the so-called astral body which votaries of an oriental belief imagine themselves able to conceive out of the depths of transcendental revery; two centuries ago Hoffmann and other anatomists spoke habitually of the vital fluid as contemporary physicists of phlogiston, and within a hundred years leading physiologists, like Hunter, thought and wrote of the "diffused vital material;" less than a quarter-century ago, Barker was deemed bold unto recklessness for undertaking to correlate vital and physical forces,[2] and many heads were shaken

[1] As defined by Le Conte in a notable article "On the Structure and Origin of Mountains," American Journal of Science, third series, vol. XVI, 1878, page 107.

[2] The Correlation of Vital and Physical Forces," University Series, Number 2 (Van Nostrand), 1875.

doubtfully when, in his presidential address before the American Association at Boston in 1880, the same brilliant experimentalist argued from the applications of Mosso's plethysmograph that mental force also may be weighed and measured, so that it must be regarded as interconvertible with other forms of energy;[1] yet half a generation of organic chemistry and physics has established these revolutionary propositions beyond peradventure, and introduced a new era of biologic research.

In the infancy of geologic science, the formations and the extinction of faunas were ascribed to extra-natural cataclysms, the opening of valleys and the shaping of hills to ill-conceived or inconceivable catastrophes; with Lyell — a personal associate of scientific men now living — came the doctrine of uniformism, under which it is recognized that existing rains and rivers and silt distributing waves are competent to produce the land-forms and formations of the earth, provided time enough be allowed them; the present generation of geologists, beginning with Powell and including two score others, have scanned the pages of the Great Stone Book so well laid open by Colorado and other rivers, and have learned to read earth-history from land-forms as well as formations, and have shown that at least a portion of those earth-crust movements which were sheer mystery to Lyell result from the slow transfer of rock-matter by the action of running water. As interpretation grew definite and the mystery of earth-making dissolved, the classification gradually changed from chiefly material or static to chiefly dynamic; for a time the formations were classified by the processes of accumulation; and now the foremost geologists classify earth-science primarily by the great agencies of earth-making.

In anthropology interpretation has not yet grown definite and there are nearly as many modes of interpreting as there are men to interpret, yet even in the short and complex history of this youngest of the sciences the general tendency appears; for the earlier classifications were based on bodily or somatic features, while the more advanced among current classifications rest either on collective attributes or on activities of the human groups — *i. e.*, the older classifications indicate what men *are*, the newer indicate what men *do*. Only half a generation past was it definitively

[1] Proceedings of the American Association for the Advancement of Science, volume XXIX, 1881, page 12 *et sequentia*.

suggested that human mentality is a form of energy, but already the testimony of the plethysmograph has been corroborated in so many ways and so widely extended that most scientific students of mental phenomena assume, either explicitly or implicitly, the essentially physical character of intellectual action; and in this writing it is assumed that intellectual energy is paramount in that it is able to control other forms of energy and make conquest of nature through invention and construction, and the faculties and works of man are classified and interpreted accordingly.

So in astronomy and physics and chemistry, and equally in biology and geology, the progress of Science may be measured by the ever-increasing recognition of the dynamic aspect of phenomena, of the physical forces by which the material things are moved; with the recognition of inherent energy or motion, observation progresses from the merely qualitative to the quantitative, and constantly increases in refinement; and in view of this progress in other sciences it can hardly be regarded as premature to attempt the extension of quantitative measure and dynamic interpretation to that side of anthropology which deals with the purely human attributes.

5. In general, scientific interpretation progresses from the stationary to the sequential; for the idea of action engenders the idea of succession. The Chaldean shepherd, the Egyptian soothsayer, and the Peruvian priest, like the earlier oriental astrologer, probably first took note of the celestial bodies as striking features of the cosmos, and later observed their rhythmic procession with such care that cycles were established and eclipses and other prodigies were foretold long before the true structure of the solar system was understood. These ancient observations and interpretations must have implanted that idea of the uniformity of nature which has borne so splendid fruit during the present century; the budding notion found poetic expression in pleasing fancies of firmaments of crystal and the music of the spheres; yet it was not until the germinal idea was fertilized by the Newtonian law that the marvellous measure of celestial rhythm came to be known. Led by the planless experiments of daily toil the mechanic — forerunner of the physicist — was the next to lay hold on the notion of uniform succession; it grew with the centuries and spread into the neighboring domain of chemistry, where it vitalized the dynamic interpretation of chemic union and aided in producing Avogadro's law, which,

according to Cooke, "holds the same place in chemistry that the law of gravitation does in astronomy"[1] and forms the basis of what has justly been called the New Chemistry. This law, like all others in science, afforded a means of prevision, or of presaging the unknown in terms of the known, and thus of testing its own validity; and as test followed test the idea of orderly succession grew until, with the aid of refined observation and the guidance of special experiment, it matured in the doctrine of the persistence of motion, the key-note of modern science. Here was a vantage point from which the astronomer was enabled to study the celestial bodies, especially our own sun, not merely as masses but as chemical and physical assemblages; and so arose the line of research sometimes called celestial physics, but defined and dignified by Langley as the New Astronomy,[2] which has already afforded a means of analyzing the constituents and measuring the movements of several among the myriads of other suns than ours. True, each of these strides in the advance of physical science represents progressive appreciation of cosmical forces; yet still more fully do they represent progress in recognizing orderly sequence and causal succession in the movements of molar and molecular bodies.

Borrowing from physical science trenchant ideas concerning force and succession, even the earlier biologists analyzed the mechanism of living things and slowly stripped away the primitive panoply of mystery or divinity in which the infantile imagination, whether of men or races, has always enveiled vitality. Lamarck was one of the first to extend the idea of orderly succession to organisms, and although his special hypothesis of development has fallen into abeyance it has features which anthropologists do well to remember; then came patient Darwin and doughty Huxley and studious Spencer with the definite doctrine of organic evolution which spread from man to man and from land to land, producing the greatest and quickest intellectual revolution in the history of the world. Albeit revolutionary, the Darwinian doctrine was but the biotic complement of the physical doctrine of the persistence of motion; and the two doctrines are twin buttresses on which the symmetric structure of modern science is supported. Through the later doctrine the world and the things thereof were transfigured in a new beauty and perfection, the universe was invested with a

[1] "The New Chemistry" (International Scientific Series, VI) 1875, page 18.
[2] "The New Astronomy," 1888, chapter 1.

new glory, and the narrow notion of breaks in the uniform course of nature by special fiat lost hold on the scientific mind forever.

It chanced that while the ferment of evolution was still fresh, a trio of American geologists (Powell, followed by Gilbert and Dutton) entered the inspiring region traversed by Colorado canyon; and before their work was done the germ of geomorphy or the New Geology was planted — it was realized more clearly than ever before that the hills are not everlasting but ever-changing, and that the features of every landscape tell an eloquent tale of continental evolution in which competent cause and commensurate effect follow ever in ceaseless succession through the eons of earth-making. The task of the geologist is not ended, indeed is only well begun; yet here as in other sciences the reign of law is realized, and the day of appeal to chance is past.

When Huxley sought "Man's Place in Nature"[1] and still more when Darwin traced "The Descent of Man,"[2] the fruitful idea of the uniformity of nature was pushed into the domain of anthropology, and has now guided for a generation those branches of the science which deal with the animal side of Man; it is true that the rhythmic sequence of cause and effect has hardly been extended so far as to cover the delicate and elusive attributes of Humanity, but this extension is the aim of many investigators, the motive of the present writing. Already the broad realm of Humanity is fairly defined, and the distinctive form of developmental succession proper to this realm is fairly outlined, so that the distinctness of the science of human attributes has been made clear; for while stellar and molecular and organic development are evolutionary in that the main tendency of change is toward differentiation, the development of humanity is involutionary in that the main tendencies are toward integration and combination. Conformably to the fundamental facts of the great realms of nature the earlier sciences are largely, perhaps chiefly, analytic, while the science of humanity is largely, perhaps chiefly, synthetic; and its votaries seem to find reason for figuring it as the central dome crowning and conjoining the separate columns in the ideal pantheon of Science. If the confidence of the votaries is just, the youngest of the sciences may be expected to repay with interest all that it received from the several branches of knowledge whence it sprang — already indeed it has thrown

[1] First published in 1863.

[2] The first edition of this notable work appeared in 1871.

light on the course of organic development through researches on the human body, and has begun to guide the acquisition of knowledge through researches on the human brain and its functions; already it is contributing to the physical sciences, *e. g.*, through the refreshing Powellian doctrine of conservation, or of common persistence of motion and matter in the ultimate particle, whereby ideas concerning the mechanism of the universe would seem to be immeasurably simplified and extended; and thére are other ways in which the youngest science is daily contributing to the stock of definite knowledge — their name is legion.

So it is that science has always progressed from the rare to the common, from the remote to the near, from the abnormal to the normal, from the simply qualitative to the quantitative, from the merely material aspect to the physical aspect, from the primitive faith in fixity to living realization of causal succession. At first sight this progress may seem puzzling, even paradoxical; yet the general course is but an expression of the order of intellectual operations pursued in scientific research. The first step is observation, which is easy when the objects observed are isolated or distinct, increasingly difficult as the objects increase in number and similarity; the second step is generalization, which is relatively easy when the objects examined are few, relatively difficult when they are many; while the ancillary process of discrimination of the incongruous likewise grows laborious with the multiplication of objects and similarities. Accordingly it is easy to study the rare, the remote, and the abnormal, and as faculty is strengthened by exercise it gradually becomes easy to progress toward the common, the near, and the normal. So also qualitative determination is easy, quantitative determination difficult — indeed exact quantitative work is impossible without careful training, as numberless surveyors and teachers can testify. In like manner interpretation in terms of the material, coupled with appeal to the supernatural when obstacles are encountered, is relatively easy and characteristic of the indolent or immature mind, while the firm grasp of analogy and homology, and the clear recognition of energy and sequence, require both native capacity and systematic training. Accordingly scientific interpretation in terms of action and succession is the end of mental effort, and may be regarded as the highest expression of intellectuality. This correspondence

between the method of research and the history of Science throughout the centuries amply attests the excellence of the method. Yet it is not to be forgotten that just as intellectual grasp strengthens so interpretation is simplified, partly through the elimination of that question-begging mysticism which pervades all primitive philosophies, partly through clearer arrangement of facts and relations; and as interpretation grows simple three especially noteworthy effects follow: (1) each step in interpretation makes the later steps easier; (2) as the labor lightens, more energy is left for the next task, and the mind of the student pushes into new fields of study and from time to time organizes new branches of inquiry; and (3) with each extension of inquiry mental faculty is stimulated and strengthened. These tendencies are clearly indicated by the birth and growth of new sciences recorded in the history of research beginning with the celestial bodies Science has extended to mechanical bodies, vegetals. animals, the earth itself, then to the human body, individual and collective; and now it is reaching out toward the special attributes of mankind. the things nearest to human welfare and happiness.

V.

The domain of Anthropology is vast, and, partly by reason of its very magnitude, is sometimes deemed indefinite; yet in the light of the history of Science in general its limits and subdivisions may easily be outlined.

History and analogy combine to show that the study of Man began with wounds and diseases and grew into surgery and medicine, which were at first thaumaturgic but gradually became rational or scientific;[1] and in this way definite knowledge of the human body gradually accumulated and Anatomy and Physiology, with various ancillary sciences and sub-sciences, took form and function. Meantime the organs of the human body were compared and identified with those of beasts and birds (which were long the better known), and comparative anatomy was established; but it was not until observation and generalization were fertilized by scientific zoölogy that the study of structures in their functional

[1] The genesis of surgery, and incidentally of medicine, is treated in a memoir on "Primitive Trephining in Peru," Sixteenth Annual Report of the Bureau of American Ethnology, 1897, especially on pages 19 and 72.

aspect took shape in Morphology. Under the influence of humanitarian therapy and the unprecedented stimulus of the Darwinian doctrine of development, the investigations of the last century, and particularly the last quarter-century, have extended from structures to functions and thence, through the fruitful science of Embryology, to human ontogeny and phylogeny — to the individual and generic evolution of Man considered as an animate organism. Accordingly there are several branches of science which deal alike with the human organism and the various other animal and even vegetal organisms of the great vital series in which Man is usually (though not invariably) considered the culminating and crowning form. Here anthropology and biology blend; but it is convenient and desirable to distinguish that division of the Science of Man which deals with the organic features of the order Bimana, and this science or subscience is frequently called *Somatology*. Although the oldest and the simplest among the divisions of the anthropic sciences, Somatology comprises various special branches of knowledge commonly classed as sciences, including Pathology, Physiology, Etiology, etc., representing the specific methods and purposes of particular classes of investigators.

The early books and maps of civilized nations show that explorers and pioneers were profoundly impressed by the far-away peoples encountered in their wanderings; there are not only accounts but pictures of headless men with faces in their chests, of cyclops with single eyes set skyward, and of other impossible monstrosities in human semblance; even since zoölogy became fairly definite, accounts of ten-foot giants in Patagonia and three-foot pygmies in Central Africa and other lands remote or hidden have gained currency and credence. As exploration continued, the unconscionable extravagancies of vision were gradually corrected, and the explorers came to see alien races in proper form and something like proper stature; yet the interest in the stranger peoples remained unabated and led to systematic observation and record. Borrowing methods from biology the observers or their interpreters sought to classify the men of different continents and provinces and islands by somatic characters — by stature, color of skin, color and texture of hair, color and attitude of eyes, form of feature, form and size of skull, peculiarities of long bones, etc., and, as the researches became definite and fruitful, they were combined in a science of races called *Ethnology*. This science has much in

common with biology, and is a direct outgrowth from that group of sciences pertaining to the human body combined under the term Somatology.

After centuries of unscientific and unsuccessful search for the seat of the soul through baseless deduction and blind introspection, certain thinkers began to profit by contemporary researches in anatomy and physiology; and as eye and mind were trained — even as the eye and mind of the traveler were trained not to make monstrosities out of unfamiliar races — the form and function of the nervous system were gradually recognized, and the dominance of the brain was finally established. Only within a generation or two has the brain been investigated in a scientific way and with due appreciation of the importance of that marvellous ganglion preformed in the articulates, potentialized throughout the long line of vertebrates, and perfected in the ultimate mammalian form of the genus Homo; yet during the present quarter-century the research has been organized in a science already cultivated in many lands and taught in most of the leading universities. The earlier promoters of this science approached the subject haltingly from the speculative or deductive side, and perhaps for this reason the science is named not so much from the organ itself as from its product *Psychology*. This modern science is not to be confounded with certain fantastic notions sometimes foisted under the same designation which do little more than obstruct progress; the parent stock of the science was indeed speculative — as is most knowledge in the beginning, — but so soon as the graft of somatology was affixed it became fruitful. It is to be noted that while somatology is essentially biotic and ethnology is biotic in so far as it rests on bodily features, psychology pushes beyond the domain of biology proper, partly in that the human brain owes its perfection of development to the essentially human attributes, partly in that the science, as commonly defined, embraces both brain and mind — both organ and product.

So there are three well-established and widely recognized sciences whose object-matter is Man considered as an organism; by some students (especially those of past decades) they are held to constitute the whole of Anthropology; by others they are combined as Physical Anthropology and regarded as including only the animal side of Man but excluding nearly the whole of the essentially human side — nearly but not quite the whole, since the field of psy-

chology is common ground. This is the view of several modern anthropologists, and is that held in this writing.

VI.

Passing from that portion of the domain of Anthropology which deals with Man as an animate genus, there is found another and still broader portion occupied by that which Man does as a sentient, volitient, and intelligent being; it is true that this portion of the domain is less definite than the other, yet, in the light of intellectual progress its limits and subdivisions also may be outlined, albeit in some measure provisionally.

The early explorers who came home laden with travelers' tales sometimes brought also more tangible cargo in the form of strange wares; and so the handiwork of the world gradually came under the observation of students, and in time museums were built partly to accommodate the constantly increasing collections of primitive and alien arts. Meantime observant persons in many lands were attracted by relics of archaic culture in the form of implements, weapons, ornaments, apparel, and habitations, as well as burial places sometimes containing the bones of the ancient artisans; and these too were collected, and museums were built to accommodate them in connection with the artificial material gathered among the living peoples of distant lands. As collectors and collections multiplied the work was organized; and although the initial stimulus came from observation in remote countries the interest grew inward — as is the way of advancing knowledge — and the local research for the rare relics of past ages was the first to receive name and character as the science of Archæology. As observations multiplied resemblances were found between the culture-products of remote times and remote places; the arts of primitive peoples were found to vary in a manner corresponding more or less closely to race, and thereby ethnic research gained new impetus and served in turn to guide research in the prehistoric; so Archæology and Ethnology became mutually helpful and grew apace and came to be intimately associated in most minds, despite the fact that the one is concerned primarily with what man is, the other with what man does; and in some circles these branches of inquiry came to be regarded as constituting the whole of Anthropology.

At first the products of ancient and alien handiwork were accepted at their token value, much like the chemic elements before

Avogadro, the planetary movements before Newton, our sun and others before the doctrine of the persistence of motion, the organic species before Darwin; but within a generation or two it has come to be realized that they possess an innate value as exponents of intellectual activity — as medals of human creation collectively attesting the birth and growth of discovery and invention, design and motive, and all other human faculties. Perhaps the time has not come for defining this stage in the progress of anthropology; it may be that the transition is not yet complete, or that the relations are too complex for easy grasp; yet it seems clear that when the anthropologist first saw in the implement of shell or stone an index to the mental operations of the implement-maker hardly less definite than the written page to the thought of the writer, the Science of Man rose to a higher plane with a bound comparable to those marking great epochs in the development of the other sciences.

Now in Science each advance gives a new standpoint from which a broader view may be gained; and with the recognition of what may be called the dynamic aspect of artificial objects, the way was prepared for further progress. It was soon perceived that the simplest devices are supplements to or substitutes for bodily organs — that the knife of shell or tooth or stone is a supplement to teeth and nails, that the hammer-stone multiplies the efficiency of blows, and that the missile is equivalent to an indefinite prolongation of reach; and accordingly it was realized that, in so far as he is a maker and user of implements and weapons, even the lowest savage rises above the plane of purely animal life. It was next perceived that even the simplest devices react on the organisms in various ways: the substitution of the shell knife for nails and teeth diminishes the exercise and hence the vigor of these organs and removes them from the category of characters subject to development through the survival of the fittest in the strife for existence, so that in so far as he employs devices in lieu of organs the savage passes beyond the realm of organic development by natural selection; at the same time the exercise of making and using artificial devices in lieu of natural organs tends to develop distinctively intellectual or cerebral characters; so that the effect of competition in the use of devices is not only to remove Man from the realm of the biotic but to set him on a definite course of development in a new realm — the realm of the artificial or essentially human. As the view of the artificial continued to broaden it was perceived that while the simpler devices may appertain to individuals they are not

integral part of the individual like the organs which they supplement, but may and do pass from hand to hand and from group to group; also that the use of a device by one person prompts others to acquire and use similar devices, which they are able to do immediately through mere exercise of individual volition (rather than slowly through generations of natural selection), so that each discovery or invention is at once the germ of a line of devices and a stimulus to intellectual power; and thus it was recognized that there is a strong communal tendency in the realm of the artificial — that the development of devices tends toward interchange and co-operation, yet ever of such sort as to augment intellectual power and elevate the human above the subhuman.

In the light of the dynamic interpretation of devices it is easy to perceive the trend of superorganic succession, or development of the artificial, and to contrast it with the course of biotic evolution. The substance or substratum of the latter is living matter, that of the former any matter, living or dead, with which man chooses to deal; the mode of this is slow elimination of the unfit and unpremeditated survival of the fit, the method of that is immediate imitation and designed improvement of the ingenious; the tendency of biotic evolution is toward organic differentiation, that of artificial development mainly toward organic persistence with endless multiplication and integration of devices; the effect of the one is individual or egoistic, that of the other communal and altruistic. With the recognition of the dynamic and successional aspects of artificial devices, Anthropology gained a new significance; for to its objects-matter in the form of the human body and human races and the human brain there was added the whole series of artificial devices and the exceeding potent intellectual activities which these devices represent — and this addition is the basis of what is here styled the Science of Humanity.[1]

VII.

When artificial devices were interpreted in terms of activities, a new classification of human handiwork arose. At the same time

[1] The enlargement of the domain of Anthropology as here set forth is regarded as marking the most important epoch in the development of this science, one of the most important in the history of Science in general. Several investigators have contributed to it; perhaps the earliest, one of the most voluminous, and certainly the most original of these contributors is J. W. Powell, whose preliminary writings have appeared in a large number of addresses, official reports, and minor papers, though his final conclusions are not yet published.

the activities themselves became objects of research, which soon passed beyond the collections and extended to the multifarious material devices in daily use among living peoples in the various stages of civilization from savagery to enlightenment; still later the research was extended to the intellectual or non-material devices which preëminently distinguish mankind, such as law, letters, and learning in their numberless aspects. The study of the activities is now sufficiently advanced to indicate, at least provisionally, their relations among each other and to the merely organic processes; they may be arranged in the order of their affinity with the vital.

1. The primary activities of mankind are connected with more or less spontaneous sensations of pleasurable character. They arise and expand in fairly definite order; among known primitive peoples they appeal chiefly to the senses, and among more advanced peoples they appeal largely to the emotions and the purely intellectual faculties; they root in sports, games, and decorations, and mature in the fine-arts of painting, sculpture, the drama, poesy, and music — *i. e.*, they constitute the esthetics. The artificial devices growing out of these activities go far towards filling those museums of the world devoted to archaic and ethnic material; and nearly a third part of current anthropologic literature is devoted to this class of objects and the activities which they represent. The activities and the activital products form the objects-matter of a broad and fruitful field of inquiry known of late as *Esthetology*.

2. Intimately connected with the primary activities, and also originating in spontaneous vital processes though becoming dominant only by organization through exercise and volition, there are other activities tending toward the maintenance of physical welfare. From a simple beginning in occupations akin to those of the beasts, they arise and expand with each step in cultural advancement from savagery to enlightenment; at first confined to food-getting, they extend also to the making of apparel, the building of habitations, and eventually to the supply of intellectual demands — *i. e.*, they constitute the industries of common parlance. The material devices growing out of the industrial activities have enriched anthropologic museums almost equally with those growing out of esthetic activities; and probably a fourth part of the current literature of anthropology is devoted to them and the activities by which they are produced. Together they form the objects matter of a large and rich science commonly called *Technology*.

It is to be noted that the greater part of the material investigated by the archæologist pertains also to the fields of Esthetology and Technology, though these are far broader in that they extend not only to a greater variety of activital products but to the activities themselves. It may also be noted that both esthetics and industries, originating as they do in vital processes, are primarily individual, though they become collective partly through combination with higher activities; while the higher activities of the series are primarily collective. It is noteworthy, too, that the two lower classes of essentially human activities rest on a material basis and are represented primarily by material devices, while the activities of higher planes rise above the material in their essential character and are only incidentally represented by material devices.

3. The activities of the third class are connected with collective relations; and since they grow out of consanguinity or family relation they may be said to have a biotic germ. In general the products of these activities originate as customs which grade into regulations and later into laws, and are perpetuated in tribal, national, and other institutions. The activities and their products are most intimately connected with, and indeed form the chief basis of, cultural progress. In the first culture stage, corresponding to what is commonly called savagery, the collective or social relation is based on kinship traced in the maternal line; in the stage commonly called barbarism, social relation rests on kinship traced through the paternal line — these stages forming tribal society. In the third stage the social organization passes from patriarchy through feudalism or an equivalent intermediate condition not yet formulated to that stage of individual property-right in lands and goods, which is commonly called civilization; and men are now passing into the stage characterized by intellectual property-right which is commonly called enlightenment—the organization in the last two stages being essentially nonconsanguineal and constituting what is sometimes called national society. The several activities and activital products belonging to this class form the objects-matter of a fecund science commonly called *Sociology* — though the day of final agreement concerning the definition of the term is not yet.

4. In some measure the activities of the fourth class are an outgrowth of those of the third, since they are essentially superorganic and may be regarded as a means of establishing and main-

taining relation; they comprise expression, pantomimic, oral, and graphic. Like the other activities they arise and expand in a certain order; beginning with what is somewhat incongruously called gesture-speech and with rudely articulate language, they mature in oratory and literature; and it is significant that the lines of development, so far as ascertained, run counter to those of biotic evolution in that they are almost wholly convergent instead of divergent — so that these activities pertain, in every essential respect, to the superorganic realm of Humanity. The principal activities are speech and writing; the tangible products are legend and literature — but the rich and ever-growing content of these products is Knowledge. The activities of expression and their products are commonly combined as the object-matter of a distinct science frequently called *Philology* — though in this case, too, there is diversity in definition and also in designation.

5. There remains a class of elusive and protean yet immeasurably potent activities which come so near the ego and are so hard to grasp and difficult to convey that it would seem almost hopeless to attempt to define them; they are the essentially intellectual activities which form the motive and burden of expression, and their products comprise beliefs, opinions, knowledge, wisdom and all other purely intellectual possessions. These activities also arise in a definite order, which is set forth incidentally in earlier paragraphs; and by most systematic thinkers they are considered to mature in science. The activities and their products are so obscure and so diverse that they have not been combined and named in the vernacular; yet they are by some students regarded as the objects-matter of one of the broadest of the special sciences, *Sophiology*.[1]

So there are five classes of essentially human activities and activital products, each so rich in phenomena and principles and so far distinct from all other classes of things as to constitute an adequate basis for a science; they are the fine-arts or esthetics, giving basis for Esthetology; industries, forming the objects-matter of Technology; organizations or institutions, affording foundation for Sociology; language and literature with their science of Philology; and the great plasma of knowledge, forming the ill-defined but all-important object-matter of Sophiology. The five fields of research pertain primarily to Man and thus represent Anthropology; yet even casual survey of their extent and character renders it evident that they pertain not at all to the animal side of Man but

[1] Sixteenth Annual Report of the Bureau of American Ethnology, 1897, page xviii.

wholly to that side which intellectual Man alone possesses — they are five sciences of Humanity. Partly to distinguish them from the three distinct branches of knowledge concerning animal Man, partly to fix their place in the body of knowledge, they have recently been combined under the term *Demonomy;* [1] and this system of organized knowledge concerning wholly human things may fitly be designated the greater Science of Humanity.

VIII.

As knowledge arises it is applied to the promotion of happiness and welfare; this has been true of unorganized and unconsciously organized knowledge throughout the history of mankind, and is especially true of definitely organized knowledge, which thereby becomes applied science. Now knowledge concerning the human activities, even while unconscious or half conscious only, reacts upon and shapes the activities in such manner as constantly to increase their potency. Some of the ways in which the science of humanity stimulates and strengthens human activities are especially noteworthy.

1. While the great domain of anthropology is divisible into an animal side comprising three broad sciences and a human side made up of five still broader fields of research, other classifications are possible, and indeed of special utility when directed toward the practical application of the science to everyday affairs — for any assemblage of facts and relations may be classified in as many ways as there are purposes of classification. Experience shows that there is a peculiar advantage in classifying certain sciences by method of research rather than by the objects under investigation. Classified in this way, anthropology comprises (1) demography, *i. e.*, the enumeration and description of men, activital products, etc.; (2) human geography (or anthropogeography), dealing with the geographic distribution of peoples and their products; (3) political economy, which is concerned primarily with applied social forces and their products; (4) history, which deals with the rise and fall of peoples and nations; and (5) philosophy, which scrutinizes materials and forces and sequences and seeks the causes of growth and decadence among human things. This classification traverses the same domain as the more general one, and serves to

[1] Fifteenth Annual Report of the Bureau of Ethnology, 1897, page xix.

bring out the same facts and relations in somewhat different light —*i. e.*, it is artificial rather than natural, technical rather than logical, subjective rather than objective, directive rather than creative; in brief, it pertains to applications rather than to original research. For certain purposes it is desirable to combine the classifications and define special fields of inquiry by the coincidence between the two, as has recently been done happily by Giddings[1] and others; for the lines of thought represented in the two systems are strengthened by interaction — the one represents science while the other may stand for statecraft or learning, and the two combine to advance mankind in knowledge and power.

2. At the outset the science of ethnology was closely affiliated with zoölogy, representing indeed little more than the concentration of biologic inquiry on a single order of animate organisms; but with the recognition of human activities, this science was raised to a new plane. The applications of demonomy in the classification of peoples and races are many and sweeping; already the natives of the western hemisphere are classified primarily by language and incidentally by other demotic features rather than by any or all biotic characters; already the great stages in human progress from savagery to enlightenment are seriated in terms of social organization in lieu of those of the bodily features with which the biologist is wont to deal; already the present-day ethnologist gives first thought to the arts, industries, institutions, languages, and ideas of the races rather than to any or all of those individual features comprised in stature and form and color; already indeed the recognition of human activities and the course of human development has served to revolutionize the science of races no less completely than the older sciences were revolutionized by recognition of force and sequence — and just as the New Astronomy, the New Chemistry, and the New Geology are distinguished, so it is meet to distinguish a New Ethnology as a science of artificially organized groups rather than of mere upright animals.

3. With the rise of knowledge concerning activities, it was perceived that the primary function and ultimate end of devices have always been to extend and increase human power, to enable man to control plants, to subjugate animals, and to evade or utilize sun and storm, *i. e.*, to make conquest of lower nature; accordingly it was recognized that, while the human characters reflect

[1] "The Principles of Sociology," 1896, page 49.

environment measurably, as the purely biotic characters do fully, it is the essential tendency and character of man to control, rather than to be controlled by, his environment. This human power did not spring into being full-fledged — indeed modern science knows no Minervan birth — but grew up slowly through the exercise and gradual strengthening of volition and the evolution of design; so primitive peoples are partially controlled by their environment while the control diminishes with successive culture stages up to that of enlightened man who dominates by multifarious devices nearly every physical force. Examination of the successive stages in emancipation from environment brings to light many significant relations; thus it is found that when two or more primitive peoples of similar culture are subjected to similar conditions of environment their minds respond in similar ways, so that similar devices are discovered or invented; and recognition of this law of human progress has served to correct some of the most persistent misapprehensions by which anthropology, like all other sciences in their infancy, has been burdened. At the same time the recognition of progressively increasing conquest over the inorganic and the organic merely has served to define and dignify man's estate at the head of all nature.

4. Although certain human characters and characteristics were already under investigation, it can hardly be said that mankind in general came into the domain of scientific inquiry until the Darwinian doctrine of evolution was accepted; accordingly, anthropologists at first regarded Man as subject to the laws of organic development through the survival of the fittest. Then came the recognition of activital development in contradistinction from organic development; and the pendulum of opinion swung back so far that most modern anthropologists implicitly assumed the human form—the form of the order Bimana, genus Homo, and species sapiens—to be fixed and final. It now appears that the pendulum swung too far; for a long series of highly significant yet but half appreciated observations indicate that. just as the human mind dominates the materials and forces of lower nature, so may it measurably control and fashion the organism in which it is embodied; already hygiene and gymnastics have improved unnumbered physiques and lengthened the days of thousands; already leading educational institutions maintain physical departments in which they undertake so to shape and strengthen the limbs and

lungs and even the heart and bone of the matriculate that he may be graduated sound in body as strong in mind — yet these indications would seem only to point the way of progress and promise still better things in human development as later generations rise.

5. The most elusive attributes of humanity are those manifested in conduct and feeling and thought; yet, paradoxically, it was these obscure products of intellectual activity that men first sought to guide and control — for in every generation in each stage of culture from the lowest savagery to the highest enlightenment, parents have essayed to train their children, while first the tribal leaders and later the sages and statesmen have semi-consciously or in full consciousness striven ceaselessly to shape the minds of the masses. So education, or the voluntary control of individual mentality for the common good, has affected profoundly the entire course of human development, and has served ever to widen the chasm separating man from the beasts. In the earlier stages of culture, as indicated by the customs of savages still living, education was limited to the lowly esthetic and industrial activity of the prime; for the primitive thinker ascribes motive, complex feeling, and all but the simplest actions to ill-conceived extraneous potencies against which it were bootless to strive. In higher savagery and in barbarism the sphere of education extended to those features of conduct involved in the maintenance of tribal relations, and was effected partly by means of habitual appeal to the extraneous potencies which were gradually crystallized in mythic systems themselves arising in a certain order determined partly by educational practice; for in much of savagery and in all of barbarism the sources of sentiment and motive are sought outside the individual and largely beyond the realm of the real.[1] With the

[1] This is explained by the law of myth. Among feral and some domestic animals, many of the works of man and certain natural phenomena of unusual occurrence are apparently regarded as uncanny, and are feared accordingly; even among the lowest humans perception is sharper than among the beasts, so that a wider variety of things are uncanny and fearsome, and in the unconscious effort for preservation of self and kind these objects are invested with mystery; in lower or middle savagery, the uncanniness and mystical potency are gradually segregated about the more capricious, *i. e.*, the self-moving or animate things, and a zoic mythology (zoötheism or animism) arises; in higher savagery and barbarism perception is still further extended, and the mystery is transferred gradually to the shifting heavenly bodies and capricious terrestrial agencies — sun, moon, nearer planets, storm-wind, thunder and lightning — which are first invested with zoic and afterward with anthropic forms and habits; and it is only in civilization that the deific concepts are spiritualized and reduced from protean multiplicity to unity or duality. This course of development represents a highly significant concentration or crystallization of supernaturalism

birth of civilization, education extended to feeling and thought, partly through appeal to ideal potencies, and there was a tendency to exalt the esthetic and neglect the industrial; and certain educational systems rose so high into the supernal or passed so far into the metaphysical as to lose sight alike of individual conduct and of the sources of real knowledge. In modern enlightenment — especially in America — the methods and purposes of training are shaped by science. and, despite the struggle of the scholastics, education is becoming revolutionized. With the recognition of an actual universe knowable through sense and reason, training becomes definite in plan and useful in purpose; with the recognition of cerebral function and of the influence of exercise in developing the brain, the scientific psychologists of the present decade have gone far in erecting a new platform for pedagogy; and with the recognition of the relations among the activities and the activital products of man. the normal course of intellectual development would appear to have been made clear — for it seems manifest that just as observation begins with the simple and proceeds toward the complex, and just as activity begins with the spontaneous and passes into the volitional, so individual and collective mentality must arise in simple and perhaps spontaneous action, to grow through habit into sentiment, and to mature through unconscious or conscious thought in definite motive. It is heterodox, perhaps in more senses than one, to affirm that motive — the noblest char-

which cannot fail to impress students of sophiology as manifested in the several culture stages; for among the more primitive peoples supernaturalism is always the more prominent and the more dominant, though at the same time the less definite. The reason for this progress is found in the development of mental operations. Primitive man, like the beasts, possesses but little real knowledge derived from experience and little capacity for organizing that knowledge in definite systems, so that most of the natural objects and processes and sequences by which he is surrounded are inscrutable to him. Now, the more unusual objects and processes representing extraordinary conditions and forces are, on the whole, the more injurious or destructive, so that by investing the unusual with fearsome and repellent uncanniness the prototype of intellectual man increases his chance of surviving and perpetuating his kind; and thereby the germ of myth is quickened in the genus through the survival of the mystically inclined Again it is the function of intellect to coördinate sense-impressions (or to interpret and classify things perceived), and this function is performed spontaneously by the aid of memory and that form of imagination involved in prevision; when the mental content of real knowledge of objects and processes and sequences is small, the hungry mind is driven to feed on the shadows of knowledge supplied through imagination, so that the mental operations of the inchoate thinker habitually exalt the unreal and it is chiefly the imaginative faculty that is strengthened by exercise. Then, as imagination stimulates memory and enkindles reason, hypothesis and inference (albeit crude and largely unreal) begin to dominate action and raise the prototype above the plane of unthinking bestiality; the

acter of humanity, — buds in spontaneous action, blossoms in subconscious habit, and attains fruition in the highest intellectual activity — whether unconscious or conscious — of which the individual or group is capable; certainly the affirmation represents complete inversion of a notion dominant in savagery, prevalent in barbarism, and gradually weakening through civilization; yet it is sustained by all that is known of the processes of acquiring knowledge, by the history of the growth of knowledge in general, and, indeed, by nearly all applied statecraft and most applied priestcraft throughout human history. The recognition of the genesis and antecedents of motive must afford a vantage point for a clearer survey of the vast field of human emotion, affection, passion, aspiration, disposition; and, at the same time, it cannot fail to give a key-note for improved education — for the still more complete control of Mind.

These are but a few of the many ways[1] in which the great science based on human activities tends to bring order out of that vast chaos of action and thought which has so long resisted analysis and synthesis — that last citadel of the unknown.

IX.

Hitherto Humanity has been the theme of poesy and romance rather than of sober science. All men have perceived that their kind possess attributes distinguishing them from the rocks and plants and beasts of lower nature, yet for the most part these

growing mind expands chiefly in the direction of the unreal or mythical, yet since even myth-burdened intellectuality elevates man and affords new means of subjugating lower nature, the more thoughtful tend to persist and perpetuate their stock; and thereby the burgeon of myth is fostered among human kind by the survival of the imaginative. So the predominant mysticism characterizing primitive peoples was a normal if not necessary feature of nascent intellectual action, and was perpetuated partly by reason of its concomitant benefits during all of the earlier stages of cultural development. Initially beneficial, mysticism and myth persisted and even increased through association coupled with intellectual indolence, as already suggested in connection with the law of development of fable (Sixteenth Annual Report of the Bureau of American Ethnology, 1897, page 22); at first a mild extra naturalism, it grew by exercise of imagination into profound supernaturalism; and as knowledge of natural law advanced the concept was gradually abstracted and driven from the real world into the realm of the supernal. Such, in brief, is the *law of myth*, as set forth in greater detail elsewhere.

[1] One of these is the control of society itself for the common good, as shown by Ward in his masterly memoir on "Dynamic Sociology," which it must suffice to mention merely.

attributes were either ignored or transfigured into a dazzling halo which defied analysis none the less by reason of its subjective character; even to-day and in the most enlightened circles of the most enlightened nations there are few willing to consider, and content to consider dispassionately, the purely human attributes; but to these few the chaos of industries and ideals, of emotions and passions, of conduct and motive, and of all other things human, falls into a simple order nearly as definite as the order recognized in each of the older sciences—the order of human activities and activital products.

Exact knowledge began with the remote and progressed toward the near; with every stage of progress it has been a power for the conquest of natural forces and conditions, has exalted intellectual mankind above all brainless or small-brained creatures, and has made continually for human welfare and happiness; and now that the methods and purposes of science are extending to the human body and brain, it cannot be doubted that these, like all other material things, will be controlled and reconstructed for the good and the glory of intelligent Man. This is the end of the Science of Humanity.

PAPERS READ.

[ABSTRACTS.]

A STUDY FROM THE OMAHA TRIBE: THE IMPORT OF THE TOTEM. By ALICE C. FLETCHER, Peabody Museum, Harvard Univ., Cambridge, Mass.

IN this study of the significance of the Omaha totem, the aim will be to set forth, as clearly as possible, first, what these Indians believed concerning their totems; and, secondly, what these totems stood for in the tribal structure.

There will be no attempt in this paper to treat the subject of totems in a world sense; the experience of many years of research within a limited area has shown the writer that close, careful studies of the various tribes and races of the two hemispheres, are as yet too few to afford sufficient evidence for a final summing up, from which to deduce points held in common, or the equally important lines of divergence, found in the beliefs and customs involved in the use of totems.

It is proper to call attention at the outset to a few of the perplexities of a research at first hand, in a matter as recondite as that under consideration. There is the difficulty of adjusting one's own mental attitude, of preventing one's own mental atmosphere from deflecting and distorting the image of the Indian's thought. The fact that the implications of the totem are so rooted in the Indian's mentality, that he is unconscious of any strangeness in them, and is unable to discuss them objectively, constitutes a grave obstacle to be overcome. Explanations of his beliefs, customs and practices, have to be sought by indirect rather than by direct methods, have to be eliminated from a tangle of contradictions, and verified by the careful noting of the many little unconscious acts and sayings of the people, which let in a flood of light, revealing the Indian's mode of thought, and disclosing its underlying ideas. By these slow processes, with the analysis of his songs, rituals and ceremonies, we can at last come upon his beliefs concerning nature and life, and it is upon these that the totem is based.

There were two classes of totems known among the Omahas: the Personal, belonging to the individual; and the Social, that of societies and gentes.

The Personal Totem.—The question first to arise is, How did the individual obtain his totem? We learn that it was not received from an ancestor, was not the gift of any living person, but was derived through a certain rite, by the man himself.

In the Legend of the Sacred Pole of the Omahas, which has been handed down from generations, and which gives a rapid history of the people from the time when "they opened their eyes and beheld the day" to the completed organization of the tribe, we are told: "The people felt themselves weak and poor. Then the old men gathered together and said; Let us make our children cry to Wa-k̈on′-da. . . . So all the parents took their children, covered their faces with soft clay, and sent them forth to lonely places. . . . The old men said, You shall go forth to cry to Wa-k̈on′-da. . . . When on the hills you shall not ask for any particular thing, . . . whatever is good, that may Wa-k̈on′-da give. . . . Four days and nights upon the hills the youth shall pray, crying, and when he stops, shall wipe his tears with the palms of his hands, lift his wet hands to heaven, then lay them on the earth. . . . This was the people's first appeal to Wa-k̈on′-da."

This rite, called by the untranslatable name Non′-zhin-zhon, has been observed up to the present time. When the youth had reached the age of puberty, he was instructed by his parents as to what he was to do. Moistened earth was put upon his head and face, a small bow and arrows given him, and he was directed to seek a secluded spot upon the hills, and there to chant the prayer which he had been taught, and to lift his hands wet with his tears to heaven, and then to lay them upon the earth; and he was to fast until at last he fell into a trance or sleep. If, in his trance or dream, he saw or heard anything, that thing was to become the special medium through which he could receive supernatural aid. The ordeal over, the youth returned home to partake of food and to rest. No one questioned him, and for four days he spoke but little, for if within that time he should reveal his vision, it would be the same as lost to him. Afterwards he could confide it to some old man, known to have had a similar manifestation, and it then became the duty of the youth to seek until he should find the animal he had seen in his trance, when he must slay it and preserve some part of it (in cases where the vision had been of no concrete form, symbols were taken to represent it); this memento was ever after to be the sign of his vision, his totem, the most sacred thing he could ever possess, for by it his natural powers were to be so reënforced as to give him success as a hunter, victory as a warrior, and even the power to see into the future.

Belief concerning Nature and Life.—The foundation of the Indian's faith in the efficacy of the totem rested upon his belief concerning nature and life. This belief was complex and involved two prominent ideas: first, that all things, animate and inanimate, were permeated by a common life; and, second, that this life could not be broken, but was continuous.

The Common Life.—The idea of a common life was in its turn complex, but its dominating force was conceived to be that which man recognized within himself as will power. This power which could make or bring to pass he named Wa-k̈on′-da.

The question arises, did the Omaha regard Wa-k̈on′-da as a supreme

being? There is no evidence that he did so regard the power represented by that word, nor is there any intimation that he had ever conceived of a single great ruling spirit.

Anthropomorphism.—The word Wa-k̈on′-da appears to have expressed the Indian's conception of immanent life, manifest in all things. Growing out of this conception was a kind of anthropomorphism; the characteristics of man were projected upon all nature: the Rock, in the rituals, was addressed as "Aged One!" sitting with "furrowed brow" and "wrinkled loins;" the Tree lived a double life in the Indian's fancy; as did the Water, the Fire, the Winds, and the Animals. This duality can be recognized in myths, in legends, in rituals, and in the paraphernalia of ceremonies, in which there is a constant confusion of the external aspect and the anthropomorphic conception. All things were distinct from man, but in the subtle bond of a common life, embodying the idea of will, or directive energy, they were akin to him, and could lend him the aid of their special powers, even as he could help or hinder his fellow men.

Will-power.—We trace the Omaha's estimate of his own will-power in the act called Wa-zhi*n*′-dhe-dhe (wa-zhi*n*, directive energy; dhe-dhe to send;), in which, through the singing of certain songs, strength could be sent to the absent warrior in the stress of battle; or thought and will be projected to help a friend win a game or a race; or even so to influence the mind of a man, as to affect its receptivity of the supernatural. Aside from the individual practice of this power, there was, so to speak, a collective energy exercised by the Ho*n*′-he-wa-chi society in the act of Wa-zhi*n*′-a-gdhe (wa-zhi*n*, directive energy; a-gdhe, to place upon;), where the members so fixed their will upon an obnoxious person, as to isolate him from all helpful relations with men and animals, and leave him to die. A similar ability to aid or to injure was imputed to the elements and all natural forms. The Winds could bring health to man; the Stone insure him long life; the Elk could endow the pursued with speed; and the Hawk make the warrior sure to fall upon his enemy. But it is to be noticed, that while man's own will was believed to act directly, without intervening instrumentality upon his fellows, the supplementing of man's powers by the elements and the animals, was obtainable only after an appeal to Wa-k̈o*n*′-da, in the rite of the vision.

The Appeal—The prayer, which formed a part of the rite of the vision, was called Wa-k̈o*n*′-da gi-ko*n*. Gi gi-ko*n*′ is to weep, from loss as that of kindred; the prefix gi indicates possession. Gi-ko*n* is to weep from want of something not possessed, from conscious insufficiency, and the longing for something that could bring happiness or prosperity. The words of the prayer, wa-k̈o*n*′-da dhe-dhu wah-pa′-dhi*n* a-to*n*′-he, literally rendered are, wa-k̈o*n*′-da here needy I stand. (A-to*n*-he is in the third person, and implies the first, as he stands, and I am he; a form of speech used to indicate humility.) While this prayer has been combined with many rites and acts, its inherent unity of name and words has been

preserved through generations of varied experience and social development of the people.[1]

Wa-ko͞n′-da was a vague entity to the Omaha, but the anthropomorphic coloring was not lacking in the general conception; the prayer voiced man's ever present consciousness of dependence, was a craving for help, and implied a belief in some mysterious power able to understand, and respond to his appeal. The response came in a dream, or trance, wherein an appearance spoke to the man, thus initiating a relation between them, which was not established until the man, by his own effort, had procured a symbol of his visitant, which might be a feather of the bird, a tuft of hair from the animal, a black stone, or a translucent pebble. This memento or totem was never an object of worship; it was the man's credential, the fragment, to connect its possessor with the potentiality of the whole species represented by the form seen in his vision, and through which the man's strength was to be reënforced and disaster averted.

Basis of the Efficacy of the Totem.—The efficacy of the totem was based upon the Omaha's belief in the continuity of life, a continuity which not only linked the visible to the invisible, and bound the living to the dead, but which kept unbroken the thread of life running through all things, making it impossible for the part and the entirety to be dissassociated. Thus, one man could gain power over another by obtaining a lock of his hair, which brought the man himself under his influence. In the ceremony of the first cutting of the child's hair, the severed lock which was given to the Thunder god, placed the life of the child in the keeping of the god. Again, when a man's death had been predicted — by one gifted to see into the future — the disaster could be averted by certain ceremonies which included the cutting off a lock of hair from one side of the head, and a bit of flesh from the arm on the opposite side of the body, and casting them into the fire; by this sacrifice of a part, the whole was represented, the prediction fulfilled, and the man permitted to live. From the ritual of the Corn, sung when the priest distributed the kernels to indicate that the time for planting had come, we learn that these kernels were the little portions which would draw to themselves the living corn. In the ritual sung over the Sacred Buffalo Hide prior to the hunt, the same idea is present, that in the continuity of life, the part is ever connected with the whole, and that the Sacred Buffalo Hide was able to bring within reach the living animal itself.

Limitation in Totems.—The totem opened a means of communication between man and the various agencies of his environment. but it could not transcend the power of its particular species; consequently all totems were not equally potent. Men who saw the Bear in their visions were liable to be wounded in battle, as the bear was slow of movement, clumsy and easily trapped, although a savage fighter when brought to bay.

[1] This prayer can be seen on page 136, Song No. 73, of Vol. 1, No. 5, of the Archæological and Ethnological papers of the Peabody Museum.—Harvard University.

Winged forms, such as the Eagle, having greater range of sight than the creatures which traveled upon the ground, could bestow upon the men to whom they came in the dream the gift of looking into the future and foretelling coming events. Thunder gave the ability to control the elements, and the authority to conduct certain religious rites.

Despite the advantages to be derived from the possession of certain totems, the inculcations given when the youth was instructed in the rite of the vision, and taught the prayer he was to sing, forbade him to ask for any special gift, or the sight of any particular thing; he was simply to wait without fear, and to accept without question, whatever Wa-k̈on′-da might vouchsafe to send him. No man was able to choose his personal totem, but it was the general belief of the people that the powerful animals and agencies were apt to be drawn toward those who possessed natural gifts of mind, and strength of will.

Nature of the Totems.—The totems of the Omahas referred to animals the Bear, the Buffalo, the Deer, the Birds, the Turtle, and Reptiles; to the Corn; to the elements, the Winds, the Earth, the Water, and Thunder. There was nothing among them which in any way represented the human family, nor was there any trace of ancestor worship; the relation between the man and his totem did not lie along the line of natural kinship, but rested upon the peculiarities in his theory of nature, in which the will and ability to bring to pass, which he was conscious of within himself, he projected upon the universe which encompassed him. The rite of the vision was a dramatization of his abstract ideas of life and nature, and the totem was the representation of the vision in a concrete form.

THE SOCIAL TOTEM AND WHAT IT STOOD FOR IN THE TRIBE.

We have thus far seen the influence of the totem upon the individual. We are now to trace it as exerted upon groups of people; in the Religious societies; in the To*n*′-wo*n*-gdho*n* or gens; and in the development and organization of the tribe.

Religious Societies. — The totem's simplest form of social action was in the Religious societies, whose structure was based upon the grouping together of men who had received similar visions. Those who had seen the Bear made up the Bear society; those to whom the Thunder or Water beings had come formed the Thunder or the Pebble society. The membership came from every kinship group in the tribe, blood relationship was ignored, the bond of union being a common right in a common vision. These brotherhoods gradually developed a classified membership with initiatory rites, rituals, and officials set apart to conduct the ceremonials.

The function of the totem in the societies was intermediate between that of the individual totem, and the totem in its final social office, where it presided over an artificial structure, in which natural conditions were in part overruled, and the people inevitably bound together. In some of the tribes of the linguistic group to which the Omahas belong, where the

political structure of the gens is apparently weak and undeveloped, the Religious societies exist and are powerful in their organization. This fact, with other evidence which cannot be detailed here owing to its complex nature, together with the similarity traceable between the rituals and ceremonies of these Religious societies, and those incident to the inauguration of gentile and tribal officers, makes it seem probable, that the training and experience, derived from the working of these earlier societies, had taught the Leaders among the Omahas and their close cognates, certain lessons in organization, by which they had profited during the formative period of the artificial social structure of the Ton'-won-gdhon or gens.

The Ton'-won-gdhon.—The word Ton'-won-gdhon, means a place of dwellings, where kindred dwelt together. There were ten Ton'-won-gdhon u-zhu—dominant, ruling Ton'-won-gdhon—or gentes, in the Omaha tribe. These gentes practised exogomy, and traced their descent only through the father. Each gens had its particular name, which referred directly or symbolically to its totem, which was kept in mind by the practice of tabu. There was also a set of names peculiar to each gens, all having the same reference, one of which was bestowed upon each child; an Omaha's gentile name, therefore, would at once reveal his kinship group or gens. This name was proclaimed at the time of the ceremony attendant upon the cutting of the first lock of hair. After this ceremony the child's hair was cut in a fashion to symbolize the totem of its gens, and each spring, until it was about seven years of age, this peculiar trimming of the hair was repeated. The teaching of this object lesson, so placed before the children, was reënforced by their training in the strict observance of the special tabu of their gentes, holding ever before them the penalties for its violation, of blindness, physical deformity, and disease.

There were religious rites peculiar to each gens in which the members did homage to the special power represented by the gentile totem. In these ceremonies, the hereditary chiefs of the gens were the priests. It is easy to see why the totem was never forgotten, why its sign was borne through life, and at last put upon the dead, in order that they might be at once recognized by their kindred, and not wander as they passed into the spirit world.

Office of the Totem in the Gens. — In the early struggle for existence, the advantages accruing from a permanent kinship group, both in resisting aggression and in securing a food supply, could not fail to have been perceived; and, if the people were to become homogeneous and the practice of exogomy continue, some expedient must have been devised by which permanent groups could be maintained, and kinship lines be defined. The common belief of the people, kept virile by the universal practice of the rite of the vision, furnished this expedient, — a device which could be understood and accepted by all, — the concrete sign of the vision, the totem of the Leader, he whose abilities and prowess evinced supernatural favor, and won for his followers success and plenty.

From a study of the minutiæ of the customs and ceremonies within the gens, it is apparent that their underlying purpose was to impress upon the people the knowledge and the duties of kindred, and, that one of the most important of these duties was the maintenance of the union of the gens. This union of kindred we find to have been guarded by the agency of the totem. The name of the gens, the personal names of its members, and the practice of tabu, — obligatory upon all persons, except the hereditary chiefs, while they were officiating in the gentile rites pertaining to the totem, — indicate a common allegiance to a supernatural presence believed to preside over the gens by virtue of its relation to the common ancestor. These rites did not imply ancestor worship, but were a recognition of the special power represented by the totem. We also find that the gentile totem and its rites did not interfere with a man's freedom in seeking his personal totem, nor of his use of it when desiring help from the mysterious powers. The gentile totem gave no immediate hold upon the supernatural, as did the individual totem to its possessor; outside the rites already referred to, it served solely as a mark of kinship, and its connection with the supernatural was manifest only in its punishment of the violation of tabu. Briefly stated, the inculcation of the gentile totem was, that the individual belonged to a definite kinship group, from which he could never sever himself without incurring supernatural punishment.

Social growth depended upon the establishment of distinct groups, and the one power adequate for the purpose, was that which was believed to be capable of enforcing the union of the people by supernaturally inflicted penalties. The constructive influence of the totem is apparent in the unification of the To*n*'-wo*n*-gdho*n* or gens, without which the organization of the tribe would have been impossible.

The Influence of the Religious Societies upon the Gens.— In the Religious societies the people were made familiar with the idea that a common vision could create a sort of brotherhood. This fraternity was recognized and expressed by the observance of rites and ceremonies,— in which all the members took part,— setting forth the peculiar power of the totem. The influence of this training in the Religious societies is traceable in the structure of the gens, where the sign of a vision, the totem, became the symbol of a bond between the people, augmenting the natural tie of blood relationship in an exogomous group. We find this training further operative in the establishment of rites and ceremonies in honor of the gentile totem, which bore a strong resemblance to those already familiar to the people in the societies. In the gens the hereditary chief was the priest, and this centralization of authority tended to foster the political development of the gens.

Related Totems.— Certain fixed habits of thought among the Omahas growing out of their theories and beliefs concerning nature and life — upon which the totem was based — present a curious mixture of abstractions and anthropomorphism, blended with practical observations of nature. Thus, in the varied experiences of disintegration and coalescing

during past generations, composite gentes came into existence through the supposed affinity of totems. Out of the ten Omaha gentes, three only observe a single tabu; the other seven were composed of sub-groups, called Ton′-won-gdhon u-zhinga (u-zhinga, a small part), each of which had its own special tabu, obligatory upon its own members only, and not upon the other sub-groups of the gens. While there was no common totem in a composite gens, the totems of the sub-groups which formed such gens, had a kind of natural relation to each other; the objects they symbolized were more or less affiliated in the natural world, as for example: in the Mon′-dhin-k̈a-ga-he gens (the earth makers), where the totems of the sub-groups represented the earth, the stone, and the animals that lived in holes in the ground, as the wolf.

The relation between the totems of composite gentes is not always patent; it frequently exists because of fancied resemblances, or from a subtle association growing out of conditions which have sequence in the Indian mind, although disconnected, and at variance with our own observation and reason.

The Totem in the Tribal Organization. — The families within a gens pitched their tents in a particular order or form, which was that of a nearly complete circle, an opening being left as an entrance way into the enclosed space. This encampment was called by the untranslatable name, Hu′-dhu-ga. When the entire tribe camped together, each of the ten gentes, while still preserving its own internal order, opened its line of tents and became a segment of the greater tribal Hu′-dhu-ga, in which each gens had its fixed unchangeable position, so that the opening of the tribal Hu′-dhu-ga was always between the same two gentes. Both these gentes were related to Thunder. That upon the right, as one entered the circle, was the In-shta′-thun-da — flashing eye — known as the Thunder gens or people. To a sub group of this gens belonged the right of consecrating the child to the Thunder god, in the ceremony of cutting the first lock of hair; another sub-group kept the ritual used in filling the Sacred Tribal Pipes. On the left of the entrance camped the We′-zhin-shte, — a symbolic name, probably meaning the representatives of anger. The We′-zhin-shte were Elk people, having in charge the Sacred Tent of War, in which the worship of Thunder, as well as all rites pertaining to war, of which Thunder was the god, took place.

It would lead too far afield to follow at great length the inter-relations of the gentes; or the dominance of position and leadership in tribal rites and ceremonies conceded to certain gentes; or to indicate the scars left upon the Hu′-dhu-ga by the breaking away of groups of kindred; or the devices used to keep intact an ancient form and order. The point to be borne in mind is, that the position of the gentes in the tribe, and the interlacing of their functions, were regulated by the ascription of different powers to their totems; and that the unification and strengthening of the tribal structure, as in the unification and strengthening of the gens, depended upon the restraining fear of supernatural punishment by the totemic powers.

In this rapid review of Omaha beliefs and customs connected with the totem, many observances have not even been mentioned, and of those indicated, the details have had to be omitted in order to keep strictly within the limits of our subject, but the fundamental ideas which have been briefly considered, will be found to underlie all rites and ceremonies within the tribe.

Linguistic Evidence as to the Totem.— We turn now to the language for further evidence as to the import of the totem.

The name of the concrete sign of the vision is, Wa-*h*u′-be, a sacred thing. The word is applied to sacred objects other than the totem, such as the Sacred Pole, the Sacred Tents, the Sacred Tribal Pipes, etc..

The name of a Religious society always included the name of the manifestation of the vision of its members; for instance, the Bear society was called Wa′-tha-be i′-dha-e-dhe, literally rendered is, — the Bear with or by compassion, that is, those upon whom the Bear had compassion. I′-dha-e-dhe implies that this compassion, this pity, was aroused by a human being making a personal appeal, either by his destitute appearance or the moving character of his supplication. Usage forbade the application of this word to any emotion excited by animal life; it could only express a feeling between man and man, or between man and the manifestation of Wa-ǩo*n*′-da. It did not represent an abstract idea, as of a virtue, but a feeling awakened by direct contact with need. In the prayer already cited as a part of the rite of the vision, the man makes a direct appeal to Wa-ǩo*n*′-da, (" Wa-ǩo*n*da! here needy I stand,") and reference to this act is made in the employment of the word i′dha-e-dhe in the term designating the Religious societies.

The name of a gens indicated its totem, or the characteristic of the group of totems in a composite gens. When the people of a gens were spoken of in reference to their totem, the word i′-ni-ka-shi-ǩi-dhe was used immediately following that of the totem, for instance: — the Thunder people, — the I*n*-shta′-thu*n*-da gens, — were called, I*n*-gdha*n* -i′-ni-ka-shi-ǩi-dhe: — i*n*-gdha*n*′, thunder; i′-ni-ka-shi-ǩi-dhe is a composite word, meaning, they make themselves a people with, that is, with thunder they make themselves or become a people. The We′-zhi*n*-shte gens, the Elk people, were called O*n*-pa i′-ni-ka-shi-ǩi-dhe, — o*n*-pa, elk; with the Elk they make themselves a people. The word i′-ni-ka-shi-ǩi-dhe clearly indicates the constructive character of the totem in the gens.

The set of names which belonged to each gens referred to the sign or totem of a family group; these names were called ni′-ki-e, — spoken by a chief, or originated by a chief. The word ni′-ki-e points to the formative period when means were being devised to transform the family into a distinct political group; it argues a central authority, a man, a chief; the individual names which he bestowed allude solely to the power behind the chief, the manifestation of his vision represented by his totem, in the favor of which he and his kindred had made themselves a people, i′-ni-ka-shi-ǩi-dhe.

The Osage equivalent of the Omaha word i′-ni-ka-shi-ǩi-dhe, is zho′-i-

ga-ra, meaning associated with. The Otoe word used for the same purpose is, ki′-gra-jhe, they call themselves.

The word for tribe, u-ki′-te, when used as a verb, means to fight. to war against outside enemies, indicating that the need of mutual help impelled the various To*n*′-wo*n*-gdho*n* (gentes) to band together for self preservation; but the order of their grouping was, as we have seen, controlled by their totems.

Summary. — In the word for tribe, in the formation of the gens within the tribe, and in the rite which brought the individual into what he believed to be direct communication with Wa-ko*n*′-da, we trace the workings of man's consciousness of insecurity and dependence, and see his struggles to comprehend his environment, and to bring himself into helpful relations with the supernatural. And we find in this study of the Omaha totem, that while the elements, the animals and the fruits of the earth, were all related to man through a common life, this relation ran along discrete lines, and that, his appeal for help once granted, relief could only be summoned by means of the Wa-*h*u′-be, the sacred object, the totem, which brought along its special line the desired supernatural aid.

It is noteworthy that the totems of individuals, as far as known, and those of the gentes, represented the same class of objects or phenomena, and, as totems could be obtained in but one way — through the rite of the vision — the totem of a gens must have come into existence in that manner, and must have represented the manifestation of an ancestor's vision, that of a man whose ability and opportunity served to make him the founder of a family, of a group of kindred who dwelt together, fought together, and learned the value of united strength.

THE STUDY OF CEREMONY. By Dr. WASHINGTON MATTHEWS, Washington, D. C.

THERE seems to be an opinion prevalent that no science of ceremony can be created and no laws of ceremony formulated. The author thinks otherwise. He believes the importance of the study of ceremony is not generally appreciated, yet it offers material for comparative study not excelled by any other department of ethnology. Religion has been well studied but more through doctrine and literature than through ceremony. The author gives several reasons why the accurate study of ceremony has been neglected and mentions some of the more important difficulties that embarrass the student both of religious rites and of the rites of secret societies. Instances are given of the great prevalence of esoteric societies and rites among Indian tribes and the silence of the Indians regarding them. Reasons are given for the decay of ceremony among our native races and instances of this decay. But while much has been irretrievably lost, there is still enough ceremony left to reward the investiga-

tor. The elaborate ceremonies of the Arickarees, in 1865, are especially referred to. Attention is called to the existence among European and Asiatic races of ceremonies which have not been studied or only imperfectly studied. The author closes by discussing different names for the science of ceremony.

[This paper will be printed in Journal of American Folk Lore.]

KORESHANITY: A LATTER-DAY CULT. By ANITA NEWCOMB MCGEE, M.D., Washington, D. C.

AN exposition of the psychological phenomenon of a comparatively intelligent body of hundreds of Americans whose beliefs are as extravagant as any folk lore or superstition known.

Their cosmology and theology are described and other features of the cult are touched on. A sketch of the founder, Dr. Cyrus Teed or "Koresh" (who claims to be divine) and of the establishment of the belief is given.

Finally, it is pointed out that by reason of the nature of the definite prophecy taught, the belief — in spite of its present success — cannot endure beyond this generation.

ORIGIN OF THE WEEK AND HOLY DAY AMONG PRIMITIVE PEOPLES. By Rev. R. J. FLOODY, South Ashburnham, Mass.

The substance of this paper to be printed in book form entitled "Scientific Basis of Sabbath and Sunday."

THE SUPERSTITIONS, BELIEFS AND PRACTICES OF THE ANCIENT MEXICANS. By ZELIA NUTTALL, Peabody Museum, Cambridge, Mass.

[This paper will be published in the Journal of American Folk Lore.]

COMPARISON OF CHEROKEE AND EUROPEAN SYMBOLISM. By Dr. STEPHEN D. PEET, Chicago, Ill.

THE JESUP NORTH PACIFIC EXPEDITION AND THE ASIATIC-AMERICAN PROBLEM. By Prof. F. W. PUTNAM, Curator of Anthropology, American Museum of Nat. Hist.. New York, N. Y.

AN ARCHÆOLOGIC MAP OF OHIO. By WARREN K. MOOREHEAD, Ohio State University, Columbus, Ohio.

FOR three years the Ohio State Archæological and Historical Society has been at work on a map of the state of Ohio, illustrative of all the prehistoric monuments in the state. At present this map contains 3,304 marks standing for 7,100 mounds, village sites, glacial kame burials, fortifications, enclosures, circles, etc. These are recorded according to townships and mile sections. The work has necessitated a great deal of travel over the state. At present, the record is only partially complete and it is estimated that several years will elapse before anything like a complete location of all the prehistoric monuments of Ohio can be made There are many of the smaller monuments which have completely disappeared and left no trace.

The map is important in that it shows the predominance of stone work over those of earth in the Muskingum Valley and Brush Creek Valley. It also emphasizes the fact that great numbers of burials were made in gravel knolls, almost as many as in the tumuli.

In point of numbers, Ross County ranks first, Pickaway second, Licking third, etc. Many of the counties in the hilly regions of eastern Ohio contain but three or four monuments each.

Mapping of prehistoric remains in Europe is common, but I am not aware that any attempt along this line has been made here in America save by the Bureau of Ethnology — and its work so far as Ohio is concerned is not accurate, mounds being mapped to a scale giving them a diameter of three miles.

The map is quite large — the monuments being a trifle larger than a pin head and covering 300 to 350 yards of territory.

THE SIGNIFICANCE OF JOHN ELIOT'S NATICK. By WM. WALLACE TOOKER, Sag Harbor, N. Y.

[This paper is printed in the American Anthropologist for September, 1897.]

THE ETHNOLOGICAL ARRANGEMENT OF ARCHÆOLOGICAL MATERIAL. By HARLAN I. SMITH, American Museum of Natural History, New York, N. Y.

ARCHÆOLOGICAL material has been arranged by the author in such a manner as to show the life and customs, or ethnology, of a people concerning whom no evidences can be obtained except from archæological

remains. This method of arranging archaeological material brings out much in relation to the physical characteristics and life of the people; their food, manufactures, methods of manufacture, materials used, their ornamental art, adornments, amusements, ceremonies, animal associates, diseases and injuries, and methods of burial.

POPULAR ANTHROPOLOGY IN MUSEUMS. By HARLAN I. SMITH, American Museum of Natural History, New York, N. Y.

THE author gave descriptions of several popular anthropological exhibits in the American Museum of Natural History, which are designed especially to interest and instruct the general public. Popular exhibits should be made without detracting from the scientific value of the specimens used, or hampering the usefulness of the scientific collection. This may often be done by use of duplicate specimens and by certain methods of graphic arrangement, including models, photographs, maps, etc. The synoptic arrangement also may be employed.

THE ORIGIN OF ART AS MANIFESTED IN THE WORK OF PREHISTORIC MAN. By THOMAS WILSON, National Museum, Washington, D. C.

THE ARTIFICIALIZATION OF ANIMALS AND PLANTS. By Prof. O. T. MASON, National Museum, Washington, D. C.

THE GENESIS OF IMPLEMENT-MAKING. By FRANK HAMILTON CUSHING, Bureau of American Ethnology, Washington, D. C.

To understand man's beginnings in implement-using and -making, some consideration of the probable condition and environment of his ancestral species is necessary.

Comparison of the human hand with the paws of climbing animals, no less than comparative anatomical studies, indicates that it was developed by climbing — that the immediate ancestors of man were arboreal or resorted to trees for protection or habitation and food supply.

As was suggested by the author in a course of lectures on Primitive Handicraft and Art delivered before the Drexel Institute of Philadelphia in the spring of 1895, it is supposed :—

First. That this primordial man must in the beginning have been confined to tropic or sub-tropic regions, those of the Malaccan straits being especially suited to sustenance in his condition.

Second. That he came, in parts contiguous to the sea, not only to resort more and more to the shores thereof, but also became, from one cause or another, restricted to shorelands (sufficiently to be compelled to use his feet for walking, his hands for *doing*, instead of for climbing and clutching merely); since only in such an environment was food supply constant enough for his maintenance in the as yet artless condition of his transition from a climbing to an upright walking species.

Third. That in such environment it was but natural for him to supplement the use of his own teeth and nails by using those of other animals, as the teeth of sharks and the sharp shells of mollusks. Such supposition is borne out by the fact that later men of like environments have returned as it were to like uses of organic materials (exclusively) for tools. This is illustrated by certain Polynesian examples, by results of recent explorations in the Gulf section of Florida, and even by the contents of shell mounds and vestigial industries in eastern Maine.

Fourth. That while it is probable the earliest implement materials, including sticks for prodding and digging, were thus organic, representing a *prelithic* stage of development, yet the use of stones for breaking and crushing is admitted to have been equally natural but not equally universal, since stones were not so universally distributed along the shores as were shells, etc. Therefore, as concerns some portions of the shoreland world, we may believe a long period of prelithic development preceded an equally long period of protolithic development in the use of natural stones elsewhere, and that this ushered in the true palæolithic period, that of the use of wrought stones, the period of real implement-making. The protolithic phase, the transition from mere stone-using to stone-working, is illustrated by the condition of the Séri Indians according to investigations of Professor W J McGee, who thus characterized them.

Fifth. That this is evidenced by analysis of the actions of children and the quadrumana as well as archæo-technic studies and experimental research from which it would appear, first, that men began in the use of stones, by pounding the things to be broken *against*, not *with* them; and, second, that when the art of pounding and breaking *with* stones was being acquired in imitation of this tendency, these were at first thrown away as soon as used, but later came to be used over and over on account of suitability, and thus found to be made by use, better and better *for* use; and, third, that therefore the first true implement-*making* began when this process of use, by which a stone had become accidentally fashioned, was imitated in the purposeful fashioning of other stones by cracking them *against*, not with, still other stones. This is illustrated by the processes of the early Tasmanians, and is the stage of human art which should be regarded, developmentally speaking, as man's *true* palæolithic stage, whatever the age in which it was developed.

Sixth. That when thus — still by adaptively imitative processes — men came to the using of these implements (made without implements) for the fashioning of yet other implements otherwise, as by percussive breaking and battering, they had already become, or speedily became, sufficiently expert in all the processes characteristic of "neolithic" art to be classified as neolithic artisans.

Seventh. That thus primitive man seems ever to have made each step in the progress of his earliest arts, by imitation of the processes of the next last step. This may be illustrated by examples of, first, the natural stick shaped by wear and further smoothed by use; second, the tool stick, that is the stick worked and smoothed *for* use in imitation of processes of mere use or of shaping and smoothing *by* use; third, the stick fashioned with other tools, the true tool stick, but still polished in imitation of mere use polishing to "age" it, that is, render it equally as convenient and effective as the stick polished by long continued wear.

Eighth. From these several considerations it is reasonable to infer, first, that in the shoreland regions of the Indian Ocean, where men presumably originated, and whence in quest of subsistence they were slowly distributed coastwise over the globe during the comparatively early periods of their art development here outlined, it is probable that they passed through a prelithic phase, used shells and wood, teeth, horns and bones of animals — as in like regions savage folk have since used them — as implements long before they used stones as fashioned tools at all; and, second, that when men of such regions penetrated regions where teeth and shells of fit kinds were not as available as stones, they had already become, through development during a protolithic or stone-*using* phase, and a palæolithic or stone-*working* phase, primordial "masters of arts" so to say — had thus acquired the art of tool-making with tools (breaking, battering, cutting and grinding or polishing), had passed the palæolithic stage as usually characterized, and moreover had probably begun the practice of making true tools of stone by fashioning these in imitation of their earlier tools of teeth, shells, etc. This is indicated by the occurrence of so many asymmetrical or tooth- and splint-like blades in regions contiguous to shorelands where like uses of organic materials have early prevailed, as in upper Florida.

Ninth. Finally, we may consider the alleged palæolithic age as representing a phase of man's development only in countries especially favorable to the art of stone chipping; namely, the countries of true flint, and probably the countries where extreme cold and the absence of good grit rock, etc., made polishing and other neolithic art processes less easy than chipping.

Illustrated by blackboard sketches; experiments exhibiting the effects of *impactive* chipping as distinguished by absence of true points or bulbs of percussion from *percussive* chipping, and with specimens of ancient Florida, Maine and Lower Potomac remains.

[This paper will be printed in the American Anthropologist or as a brochure.]

THE BUILDING OF A ZAPOTEC CITY. By Prof. W. H. HOLMES, U. S. National Museum, Washington, D. C.

THIS paper described the construction of a building in the ancient city of Mitla whose ruins are among the most interesting in Mexico. The erection of the present great buildings of stone was of comparatively recent date and the work of constructing even a single building must have been very great indeed considering the primitive characters of Zapotec culture. The design was probably worked out in great detail before construction began. The material forming the platform and the body of the thick walls consisted of stones gathered from the slopes and bluffs laid in adobe mortar. The wall facings and numerous panels of geometric mosaic ornament were cut and laid with the greatest accuracy, each stone being especially cut and fitted to its place. Even the geometric designs, the building of which in a single structure required seventy-five or eighty thousand pieces of accurately cut stone, did not require uniform shape as do our tiling and brick work, but each stone had its individual shape and individual fitting. When the walls had reached the height of sixteen feet wooden beams were laid across and covered with masonry and cement forming the roof. In the construction of a single building, such as that of the hall of the columns, eleven great stones weighing from three to eight tons were required for lintels and columns. These were cut out of the solid rock mass on the mountain sides with rude stone picks and transported several miles. The entire work of dressing and carving was no doubt accomplished with the aid of stone tools alone. The great lintels were sculptured or covered with designs in color and the entire surface of the buildings was finished in color.

RECENT RESEARCHES, BY GEORGE BYRON GORDON, ON THE BANKS OF THE ULOA RIVER IN HONDURAS, FOR THE PEABODY MUSEUM. By Prof. F. W. PUTNAM, Curator Peabody Museum, Harvard Univ., Cambridge, Mass.

[See Memoirs Peabody Museum, No. 4.]

THE GEOGRAPHICAL DISTRIBUTION OF A CERTAIN KIND OF POTTERY IN MEXICO AND CENTRAL AMERICA. By M. H. SAVILLE, American Museum of Natural History, New York, N. Y.

THE SERPENT SYMBOL IN NICARAGUA. By Dr. STEPHEN D. PEET, Chicago, Ill.

[This paper will be printed in the American Antiquarian.]

DECORATION OF THE TEETH IN ANCIENT AMERICA. By M. H. SAVILLE, American Museum of Natural History, New York, N. Y.

PREHISTORIC IMPLEMENTS FROM CHARLEVOIX, MICHIGAN. By H. P. PARMELEE, Charlevoix, Mich.

AN EXPERIMENTAL ANALYSIS OF THE RELATION OF RATE OF MOVEMENT TO CERTAIN OTHER MENTAL AND PHYSICAL PROCESSES. By Dr. LIGHTNER WITMER, University of Pennsylvania, Philadelphia, Pa.

THESE experiments concern themselves with the following groups of persons: adult men and women, women college students, Normal School students (women), Kindergarten pupils (boys and girls) and high grade idiots and imbeciles (male and female). The results are analyzed to show differences in intelligence, age, height, weight, lung capacity, rate of movement, reaction time to sound, sensitivity to pressure and to pain. Sex differences more or less pronounced manifest themselves: males in all groups have greater lung capacity, a shorter rate of movement and reaction-time and a higher threshold for pressure and pain than females of same age, intelligence and weight. In the different groups, the more intelligent (determined by the method of indices) have a more rapid reaction and a better lung capacity. College students show the best members of the class intellectually to have a slower rate of movement and a higher pain threshold than the poorest members. Among the feeble minded, the best intellectually are the quickest in reaction, in movement and the most sensitive to pain. Thus the interrelation of associated characteristics for each group of persons is an individual one, and generalization from one group to another is not always justified.

A STATISTICAL STUDY OF EMINENT MEN. By Prof. J. MCKEEN CATTELL, Columbia University, New York, N. Y.

THIS paper was introduced by the statement that the science of anthropology should concern itself not only with the beginnings of human development but should also apply the methods of genetic and exact science to its highest results. The method was explained by which a list of the one thousand most eminent men had been secured, and the list, in which the names were arranged in the order of eminence, was presented. Charts were then shown exhibiting the secular and racial distribution of the most eminent men, and attention was called to certain points brought out by the curves.

ANTHROPOLOGICAL INVESTIGATIONS CONDUCTED IN CONNECTION WITH THE PATHOLOGICAL INSTITUTE OF THE NEW YORK STATE HOSPITAL. By Dr. A. HRDLICKA, New York, N. Y.

THESE investigations comprise comparative studies of all points of importance, anatomically and physiologically, and also of certain psychological questions, on most of the important social classes of the population of New York State; namely, on the normal, the insane, the criminal and criminal-insane, on imbeciles, idiots and epileptics; and, if possible, also on prostitutes and similar subjects.

The whole field of investigations has been outlined by the author, and certain groups established (see Bulletin of the State Hospitals, New York, Jan. 1897). The whole then has been divided into groups, each one of which comprises a number of items which form the basis of the inquiries and of the examinations. Each group is so arranged as to comprise a certain number of anatomical, physiological, sociological, and also pathological, points, and to consist of proper grades of anamnestic work and of mechanical examinations.

The work is not carried on by the author alone but under his direction, by a large body of physicians who are interested in the investigations and have the various abnormal classes under observation.

At the present time the first group of the outlined studies nears its completion. This group consists of about one hundred various points of inquiries and examinations, arranged as stated above. The records will cover about eight thousand individuals. About thirty physicians have coöperated with the author on this group. The second group will probably be started during the second half of 1898.

Besides investigations on the living, a collection of anthropological material is inaugurated at the Institute. A special feature of this work will be the establishment of "anthropological cemeteries" in connection with the various state institutions, under the direction of the author.

A TREPHINED SKULL, TARAHUMARE, FROM THE NORTHWESTERN REGION OF MEXICO. By Dr. CARL LUMHOLTZ AND Dr. A. HRDLICKA, American Museum of Natural History, New York City.

THE skull in question was found by Dr. C. Lumholtz in an old closed cave, in the land of the Tarahumare, Chihuahua, Mexico. It is now in the American Museum of Natural History.

The skull was totally devoid of flesh, but the bones still retain some animal matter. The precise age of the specimen can not be computed. It is certain that the skull is not modern.

The skull is that of an aged woman and is in every respect normal; its

measurements correspond very closely to those of typical Tarahumare, and there can be no doubt that the woman belonged to this people.

The trephining is situated in the anterior and upper part of the right parietal, and consists of an almost circular opening 2 x 2 cm. in diameter. The edges of the opening are vertical, suggesting some form of boring as the method employed in making the opening. The operation was performed ante mortem and there are positive signs that the subject survived the operation several years, at least. There are no indications as to why the operation was performed.

No information as to the operation could be obtained from the Indians of the district in which the skull was found.

[Since this paper was written, another trephined skull of a Tarahumare woman has been found by Dr. Hrdlicka in the part of the same collection which is now in the Museum of Science and Art in Philadelphia.]

A DESCRIPTION OF AN ANCIENT SKELETON FOUND IN ADOBE-DEPOSITS IN THE VALLEY OF MEXICO. By Dr. HRDLICKA, New York, N. Y.

THE skeleton was found by Indian workmen about three metres deep in adobe-deposits, at St. Simon, a suburb of Mexico. It was secured by Doctor Lumholtz for the American Museum of Natural History. In about the same depth with the skeleton there were discovered other osseous remains, mainly skulls, and some very simple specimens of an archaeological nature. The skulls and other bones found show very close morphological relation with similar parts of the skeleton in question. As to the archaeological specimens of this and similar adobe-deposits, they are found in strata, and, according to Prof. Wm. Holmes, these specimens increase regularly in simplicity and rudeness as we proceed from above downwards. The uppermost layers yield fragments of pottery of Aztec character; further down the specimens lose this character and assume gradually that of very primitive peoples. The skeleton was found in these lower layers.

On examination the skeleton is found to possess the following points of interest: It belongs to a male adult, approaching 1.60 m in stature, or about the general medium. The skull is deformed in the way of a compression of the inion-obelion region. It is small in size and capacity. Of the constituents of the thorax, we find a pair of supernumerary ribs, a blending (congenital) of the first and second ribs on the left, and consequent deformities of the sternum and of the dorsal portion of the spinal column. In the upper limb the antebrachial radio-humeral index is very high, approaching closely to that of anthropoids. The pelvis is small, the sacrum composed of six segments. The tibiofemoral index approaches again closely that of anthropoids. The heads of the tibiæ are inclined considerably backward.

The racial character of the skeleton is doubtful. It is not Aztec, so far as can be asserted from our anatomico-anthropological knowledge of this people. It is almost without a doubt a pre-Aztec skeleton, belonging to a meso- or dolichocephalic race, whose true nature and ethnic position it is at present impossible to determine.

THE MANGYAN OF THE PHILIPPINES. By Prof. DEAN C. WORCESTER, Ann Arbor, Mich.

THE TAGBANUAS OF THE PHILIPPINES. By Prof. DEAN C. WORCESTER, Ann Arbor, Mich.

On Wednesday afternoon, August 11, a joint session was held with Section E for the presentation of reports by archaeologists and geologists on the recent investigations in the Delaware valley. The following papers were presented and discussed:

EARLY MAN OF THE DELAWARE VALLEY. By Prof. F. W. PUTNAM, Harvard University, Cambridge, Mass.

MR. PRESIDENT, and Members of the Sections: It falls to me to make a general statement of the work that has been done in the Delaware valley. It was in the year 1875 that Dr. Charles C. Abbott sent to the Peabody Museum several stone implements found by him in the gravel at Trenton. In the following year he sent to the Museum a report on the discovery of supposed palaeolithic implements in the glacial drift of the Delaware valley. This is printed in the Tenth Report of the Museum. This paper aroused an interest in the subject, and it was generally accepted that these implements were of the same character as those found in the gravels of the old world and that they were probably of the same age.

Later, it was said that these implements, found in the gravels of Trenton, were not implements in the true sense of the word, but were rejects in the manufacture of implements of a higher character. Then the geologists began to take exception to some of the statements that had been made in relation to the age of the gravel. It was also claimed by some persons that the chipped stones had all been found on or very near the surface; or, if any had been found several feet below the surface and in the gravel, they got there by some accidental means. Then the discussion grew a little warm, then a little warmer, and finally it became quite hot and personalities unfortunately entered into it.

I had examined the Trenton gravel on several occasions, and had hunted in vain for specimens. In 1876 I made another search and succeeded in finding, behind a little boulder in the gravel — in what I believed to be undisturbed gravel — one of these chipped stones.

In 1883 Doctor Abbott came to the conclusion that while on the surface, that is in the first foot or eighteen inches of the surface deposit, there were many implements made of chert, quartz and jasper, also bone implements and various other objects belonging to the recent Indian occupation; yet in the lower soil, beneath this black soil and undisturbed by the plough, there were many implements made of argillite and very few implements made of any other stone. It was only occasionally that chipped stones or chips of quartzite and of chert were found.

During these explorations many specimens have been collected. In the Peabody Museum at Cambridge you may see thousands of these specimens obtained under different conditions;—including those from the surface and from the glacial deposits.

From the time Doctor Abbott first mentioned the existence of what he called an argillite period, antedating the time of the Indian occupation, the evidence, it seems to me, has been steadily accumulating. Doctor Abbott associated this pre-Indian occupation with the Eskimo. We will not discuss this point — it is not essential to the question.

For over twenty years, in connection with Doctor Abbott and Mr. Volk, I have been directing explorations in the deposits in various localities about Trenton. Several thousand dollars, contributed by friends of research, have been expended in this work We have been digging, digging, digging. Sometimes we dig a long time without finding anything; at other times we do find something. This research was first carried on by the Peabody Museum, then a small amount of work was done for the World's Columbian Exposition; then, after the Exposition, the work was continued under my direction for the Peabody Museum; and now for the past two years for the American Museum of Natural History in New York City. For this recent work one thousand dollars was contributed by the Duke of Loubat.

During the past two years extensive excavations have been made in a field belonging to the Misses Lalor. These estimable ladies have very kindly granted to me the exclusive right of exploration on the farm as long as I wish. We owe them a great deal for this privilege. The outline of this field is shown in the diagrams on which is given the location of the recent trenches which will come up for consideration this afternoon. This Lalor farm is situated on the southern limits of Trenton, upon a bluff, and the field inclines a little backward from the bluff. In that field we have been digging trenches, considerably over half a mile in length and fifty feet in width. I have here a few photographs which I will pass around as they will give an idea of the place and help you to understand what the following speakers will have to say. Here is one photograph showing the field; the red arrow on the sky-line of the field

points to the bluff. Here are photographs showing the position of two of the trenches, and these show the two boulders lying in the black soil and resting on the sand which immediately underlies the black soil. Here are several pictures showing sections of the trenches and the implements as they were found in place. These specimens on the table are those shown in the photographs. This, No. 47, is the chipped stone that was found two feet down in the sand. Here is an argillite flake, No. 52, and the photograph shows that it was about eighteen inches down in the sand. No. 44, a well-chipped argillite point or knife, was about eighteen inches down. This is the type of implement so abundantly found in the deposit below the black soil. If you will dig a trench anywhere in that region, you will find many more implements of this character in the upper bed of sand than in the black soil; you will find that they are most abundant just at the border line of the black soil and the sand. This is the lot of implements and chips found, as shown in the pictures, in the excavations made by Mr. Volk in April and May. Here are samples of the sand from the several layers, and here are some of the little pebbles from one of the red bands which alternate with the sand.

I have here a number of photographs which are of great importance because they were taken in the presence of a number of archaeologists and geologists. For my own part, I am satisfied that all these specimens were found as photographed and as stated by Mr. Volk in his field notes; but in order that others might be satisfied, an invitation was given through Professor Wright to a number of archaeologists and geologists to visit the place and have trenches dug under their own personal supervision that they might see the character of the deposits. As a result several archaeologists and geologists were present on the 25th, 26th, 27th and 28th of June. The trenches were dug by Mr. Volk and his assistant, according to the direction of the party on the field. I purposely stayed away. I desired those present to have full sway to do as they wished. The trenches were dug, and here is a photograph of one of them. Here is a photograph showing a specimen in place; it is simply a stone chip, and is one of the few chert objects that have been found here. This [showing both the photograph and specimen] is a flake of argillite found by the same party on June 28th.

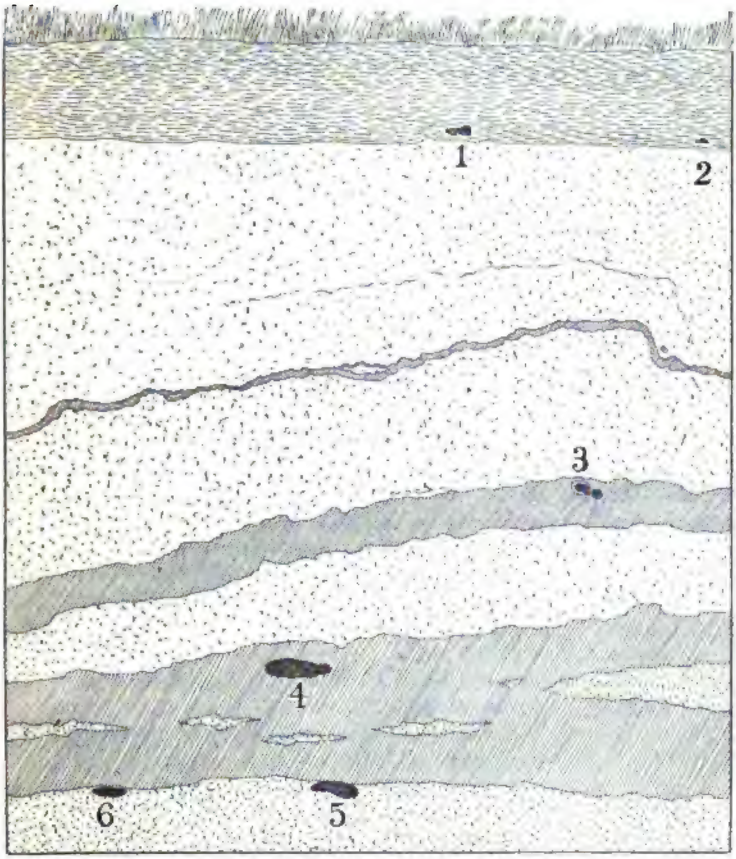
I wrote to Mr. Volk to dig another trench and to draw a diagram of it so as to show, in colors, the layers just as he saw them. The diagram is drawn to a scale of one-half.[1] The trench, four feet in length, shows the several deposits and the black soil above. Here is a photograph of the section shown in the diagram.

On one of the photographs you will notice an angular boulder partly covered by the black soil, and resting upon the sand. I requested Mr. Volk to cut a trench directly under the edge of that boulder. Here is the photograph showing the boulder resting on the sand.

It is beyond any reasonable doubt that these specimens, and a number

[1] The diagram referred to is reproduced in the accompanying figure.

of others in the New York Museum and in the Peabody Museum, were actually found *in situ* in this deposit. We know that many of these are chipped stones, and others are flakes struck off by the agency of man. We are satisfied of that; and I, for one, am perfectly satisfied that the objects are of the same age as the deposit in which they were



SECTION OF DEPOSITS AT LALOR FARM. 1/12

The shaded lines represent the layers of red clay. 1, 2, Argillite chips 3. Pebble and argillite chip. 4, 5, 6, Large pebbles.

found. Whatever the age of the layers of red clay or of the sand between them, that is the age of these implements. That age is what the geologists must determine for us. Every archaeologist knows that man has been on the earth many thousand years. That this region of the

Delaware valley was inhabited by man in very early times is beyond doubt; for there are thousands upon thousands of his implements as well as other indications of his occupancy, from the time of the deposit of these layers of sand and clay to historic times. He must, moreover, have been somewhere on the continent, while these early deposits were forming, to have reached this spot at the close of the glacial period when the region became habitable. It is for the geologists to tell us the age of these deposits.

In my remarks this afternoon I have endeavored to present, as briefly as possible, the present status of this important research in the Delaware valley. This research was undertaken, and will be continued, with the determination of making so thorough an investigation as to ascertain the actual facts,— facts which will tell their own story, whatever that story may be. [From a stenographic report.]

THE AGE OF THE ARTIFACT-BEARING SAND AT TRENTON. By HENRY B. KÜMMEL, Chicago, Ill.

ON three different occasions during the past summer I examined the deposits on the Lalor farm at Trenton, in which numerous artifacts have been found. So far as my observation goes, nothing was seen to prove that they were not *in situ*. In all cases noted they were found with longer diameters horizontal, *i. e.*, in the position they would naturally occur if their age is the same as that of the sand in which they are found. No positive evidence was noted that the sand had been so disturbed that they might have been intruded from above. On the other hand, they all occur within less than four feet of the surface, in the zone in which the sand may repeatedly have been disturbed by uprooting of trees, burrowing animals and Indian burials. Nothing of structure was seen in the sand itself by which this crucial question could be positively determined. The "red clay films" observed at various intervals in the sand are not, in my opinion, lines of stratification at all, nor are they always strongly clayey. They are partly, at least, zones or bands of infiltration and deposition of ferric oxide which has somewhat cemented the sand grains. Since they are not lines of stratification, the fact that they are continuous above the specimens is not necessarily conclusive proof that the latter are *in situ*. Nevertheless, in spite of the absence of decisive evidence *pro* or *con*, I am inclined to the view that the artifacts are *in situ* and not intrusive.

The deposit in which they occur is, in my opinion, dune-sand, accumulated after the river had partially or completely excavated its trench below the level of the Trenton terrace The reasons for this conclusion in brief are as follows:

1. *The location.* The trenches are all within one hundred or one hundred and fifty feet of the edge of the terrace, which here overlooks a broad sandy flood plain. According to the testimony of those who have

explored most thoroughly, the artifacts are found most abundantly in the sand near the edge of the bluff. The location is one peculiarly favorable for the accumulation of wind-blown sand driven by southerly and westerly winds and derived from the steep face of the terrace before it was covered by vegetation. As the river eroded its channel below the terrace level and left bare the freshly cut bank of sand and gravel, the prevailing winds undoubtedly swept sand on to the terrace. Naturally, some would accumulate along the edge of the bluff. I have observed wind-blown sand at many points in exactly similar positions farther north along the Delaware.

2. *The topography.* In the immediate vicinity of the trenches the surface of the terrace is slightly irregular, being diversified by low swells and saucer-like depressions. The surfaces of the swells are more sandy than of those parts of the terrace where the undulations are not present. Occasional large boulders occur on the surface of the terrace, but none were noted on the sandy knolls. Certainly none occur in the immediate vicinity of the trenches. In saying that the surrounding topography is at least *suggestive* of wind action I am not overstating the facts. In this connection, too, it should be noted that the present flood-plain is marked by low dunes now in the process of formation, and the similarity of surface is thus clearly brought out.

3. *The deposit.* Beneath the layer of forest loam, measuring six to ten inches in thickness, there is loose yellowish sand absolutely without structure lines, but traversed by two or three more or less distinct films, which, as noted above, are probably due to infiltration of ferric oxide. Beneath the yellowish sand, which has a maximum thickness of less than three feet, there is a reddish layer of sand, eight or ten inches thick, grading downward into the cross-bedded sand and gravel, which all are agreed is of glacial age and in which the artifacts are not found. In the artifact-bearing sands there is no evidence that it was water-deposited. That it is a local deposit is shown by the fact that it does *not* occur on the gravel seen in large open pits a few hundred yards distant from the trenches. It seems to be best developed along the edge of the terrace. Its texture is not unlike that of wind-blown sand observed elsewhere, and it is decidedly unlike the sand beds exposed in the gravel pits. This latter fact does not necessarily separate it from the glacial deposited beds, although it points to such a separation.

If it is wind-blown sand, then the reddish layer between it and the cross-bedded sand and gravel probably represents the upper surface of the Trenton gravel and was the terrace surface during the interval between the accumulation of the glacial gravel and the wind-blown sand. This layer was examined very carefully in the hope of finding proof of its being an old soil. No humus staining, however, was observed, and its absence may be an argument against the view here advanced. It is not a fatal objection, however, since its absence can be satisfactorily explained by the oxidation and leaching which the whole mass has undergone. This action is still going on, for the humus staining is being

leached out of the underside of the present soil, as is indicated by its mottled appearance through a zone five or six inches thick.

4. The presence of at least one wind-eroded pebble in the sand lends some strength to this interpretation, although in the light of the studies of Davis and Woodworth on Cape Cod it cannot be regarded as conclusive.

The presence of scattered pebbles in the sand, too large to have been moved by the wind, may at first sight seem to be fatal to this view, but when all the facts are considered it is not so. That man was present is indicated by the artifacts found. The bank from which the pebbles may have been carried by human agencies is hardly more than a hundred feet away. Although the presence of the pebbles may, to some degree, weaken the argument it is not fatal to it.

My conclusions are, therefore, that the artifacts are probably found *in situ*. There is no positive evidence that the sand deposit is water-laid, and there are strong reasons, although perhaps not conclusive, that it is wind-blown. In the latter case it may date from a period much later than the accumulation of the Trenton gravel. It seems most reasonable to suppose that it had accumulated after the river had cut its channel somewhat below the level of the terrace and formed a freshly-cut bluff, from which the sand was derived. The localization of the sand along the present bluff and the reported greater abundance of the artifacts in the sand nearest the bluff support this conclusion.

Substantially these same conclusions were reached by me at the time of my first visit to this locality, and my later observations served only to confirm them. In a letter to Professor Mercer, written about July 1st, I stated this view as to the origin of the sand, and the same conclusions were expressed to Professor Smock even earlier. Even since my first visit to this locality I have been of the opinion that these deposits are probably æolian and that they certainly do not represent the closing stages of the Trenton gravel.

ON THE IMPLEMENT-BEARING SAND DEPOSITS AT TRENTON, N. J. By G. N. KNAPP, Prairie du Sac, Wisconsin.

THE opinion is expressed that the sand deposits mentioned in the title were of wind origin, accumulated since the river had cut its trench below the level of the upper Trenton terrace. This opinion is based on a recent visit to the spot.

ON THE ORIGIN AND AGE OF THE RELIC-BEARING SAND AT TRENTON, N. J. By Prof. ROLLIN D. SALISBURY, University of Chicago, Chicago, Ill.

THE locality where human antiquities have recently been found near Trenton, New Jersey, is situated about two miles south of the heart of

the city. The points where the finds are being made are on a somewhat extensive plain, the principal formation of which is composed of the sand and gravel deposited by the glacial drainage which came down the Delaware during the last glacial epoch. On the east side of the Delaware the plain extends about two miles east of the locality where the finds are made. It also has a considerable development on the west side of the river, and extends many miles up and down the Delaware north and south of the locality in question. From Trenton it also stretches northeast a number of miles along the Assanpink creek. In the vicinity of Trenton this plain has an elevation of fifty to sixty feet. Through it the Delaware has cut a wide valley, the flood-plain of which is now less than ten feet above sea-level. The relation of the flood-plain to the plain above shows that, after the latter was made, the river excavated a valley in it, cutting it down essentially to tide-level. This valley has been cut since the last glacial epoch. The gravel plain to the east and north of the point where the finds are made ends abruptly at the margin of the post-glacial valley, in a bluff about forty feet in height, with a slope which is about as steep as the material of which it is composed will lie.

The relations shown on the New Jersey side of the river are in a general way duplicated on the Pennsylvania side. The gravel of glacial age has a similar disposition, but the border of the valley on that side is not so sharply defined, indicating that the more recent cutting of the stream has been on the east. The steepness of the bluff of gravel at the points concerned is in itself proof of the recency of the excavation on this side.

The surface of the plain is slightly undulatory, though the relief is usually but a few feet. In places erosion has affected it to some slight extent, and in places its surface appears to have been left slightly uneven by the deposition of the material of which it is made. Its surface is also characterized at various points by low mounds and ridges of sand, heaped up by the wind. By this means an element of undulatoriness has been added to the surface as originally left by the deposition of the main body of sand and gravel involved.

While the plain consists of sand and gravel, so far as its general constitution is concerned, its surface is in many places coated with a thin layer of sandy loam, which contains occasional pebbles similar to those which make up the body of the gravel beneath. It is not always possible to say to what extent the surface loam represents the last stage of deposition of the glacial sands and gravel; to what extent it represents the surface accumulation of loamy matter brought up from lower levels by the action of biotic agencies, such as worms, ants, burrowing mammals, etc.; or to what extent it represents deposition by marine or estuarine waters which stood over the region after the glacial drainage ceased to flow through this part of the Delaware.

Relations similar to those where the human relics just south of Trenton are found characterize the east side of the Delaware for many miles farther south. In this direction materials derived from glacial waters are less readily identified at most points, but the topography and relations of

the plain bordering the Delaware are such as to show that it was developed contemporaneously with the plain at Trenton. Even where not made up chiefly of glacial materials, the plain farther south, like that at Trenton, is slightly undulatory, and is coated, in places, with dune sand. Such sand is especially likely to be found on the west edge of that part of the plain which lies east of the Delaware, and just east of the line where the plain descends with a bluff face to the flood-plain of the stream. Well-marked dunes sometimes appear in this situation, and dune sand in larger or smaller quantity is so general that its presence along the edge of the plain above the valley may be said to be the rule, rather than the exception, between Trenton and Camden.

The same is true of the tributaries which come down to the Delaware from the east. Although they did not bring down glacial sands and gravels, they brought down sands and gravels of other sorts, partially filling their valleys, which, like the Delaware, have been re-excavated since. On the bluffs of the tributary valleys, as well as along the main stream, dune sand is of frequent occurrence. In the dune sand along these tributaries, relics of early peoples, consisting of chips of argillite, arrowheads, and half-fashioned tools of various sorts, are frequently found.

Sand is found in similar relations at some points on the Delaware above Trenton. At many points it has been blown up from the glacial gravel terraces to higher levels, though it rarely takes the forms of distinct dunes. It is frequently three to five feet in depth, facing the bluffs above the glacial plain in irregular patches, or capping their crests.

The trenches in which the human relics near Trenton have recently been found are upon the immediate edge of the plain overlooking the post-glacial valley of the Delaware. Here, as is frequently the case in such situations, the sandy loam over the gravel of glacial age is thicker than farther back from the bluff, but even here it is but three or four feet in thickness, including the black soil. It is in this sand and loam, quite above the materials which are clearly of glacial age, that the human relics are found.

In detail the sections shown in the trenches open in May and July showed a sandy soil affected by organic matter to the depth of six to twelve inches, the lower limit being ill-defined. The soil graded down into sand which was essentially free from organic matter, and which had a thickness of two to three feet. The sand was without apparent stratification. Below it lay the stratified drift, confidently referred to the time of the last ice epoch. It will be seen, therefore, that the relics were found in the structureless sand and loam which overlay the sand and gravel of glacial age.

Besides being essentially structureless, the sand and loam in which the relics were found contained occasional pebbles. Some of them were as large as one's fist, and occasionally one was found of still greater size, though most of them were tiny pebbles. Many of them were so small as to be within the power of wind to transport, while others were so large as to make this mode of transportation impossible.

In the sand there were at some points streaks more highly colored than the portions above or below. These streaks had a position approaching horizontality, but in detail they were exceedingly irregular. Locally they were interrupted, apparently broken; and in other places they faded out altogether. In general they were thin, a trifling fraction of an inch in thickness. They were sometimes so faint as to be traceable with difficulty, while in other places they thickened to a quarter of an inch or more. While these streaks were often distinct, they were not to be mistaken for lines of stratification, with which they clearly had nothing to do. They could not be assumed to be the edges of stratification plains distorted by unequal sinking, for if this were their origin successive streaks in the same vertical section should have corresponded in their irregularities. This was not the fact, for one streak was liable to bend up just where the one a few inches below it bent down, a relation which excluded the idea of unequal settling. Furthermore, they were so irregular that their total length, as seen in the face of a trench, measuring all irregularities, was considerably greater than the length of the section itself.

These reddish streaks, which were thought to carry more pebbles than the other portions of the sand, seemed to be due to one or more of two or three causes. In places they seemed to be due to the concentration of coloring matter, especially iron oxide. In other places they looked rather as if fine reddish silt had accumulated along them through the influence of percolating water. In either case there must have been something in the texture along this irregular surface to occasion the concentration. The surface of which these irregular lines were the outcrops may perhaps once have been the upper surface of the land, subsequently buried by wind-blown dust and sand. Many of the little irregularities of the streaks were such as might be thus explained, though the abrupt breaks in them must be accounted for in some other way. So far as I could make out, there was nothing except these reddish streaks which could by any possibility be mistaken for structure, and had I not known at the time of my second visit that they had been taken by others for the edges of stratification plains, I should not have supposed this interpretation a possible one.

Concerning the age and origin of the sand which contains the relics, no positive affirmation can be made, and it is only fair to say that this statement is made on the basis of a somewhat full knowledge of the surrounding region. So far as its stratigraphic relations are concerned, the relic-bearing sand might represent the last phase of deposition by glacial waters, or it might belong to any later epoch. Its absence of structure does not show that it was not deposited by water, for in the nature of the case it could not now be expected to show structure, whatever its origin. This would be true whether it represents (1) the last phase of deposition by glacial waters, (2) an estuarine deposit of later age, or (3) æolian sand; for the continually renewed perforation of the sand to the depth of several feet by the roots of plants, the continual borings of burrowing mammals, worms and insects, all of which frequently go down

to the bottom of the sand overlying the gravel of glacial age, would quite probably have destroyed all traces of stratification which the sand may once have had. If this were not enough, the freezing and thawing, and the wetting and drying, would have completed the obliteration of any original structure. For this result even a very few centuries would suffice. It cannot be asserted, therefore, that the sand was not once stratified.

On the other hand, the sand in which the relics are found may have been blown to its present position. The fact that the immediate edge of the bluff is slightly higher than the plain farther back lends color to this view, but the rise next the edge of the bluff is very slight, and the conclusion that it is due, at this particular point, to an accumulation of wind-blown material is not necessitated. The explanation of æolian sand in this position would be easy. While the river was cutting its valley in the plain, the bluffs were bare. The bare face of the bluff was made of loose sand and gravel, and the prevailing westerly winds might well have blown sand from the slope to the top of the bluff above. This is just the situation in which dune sand would be expected to accumulate under such circumstances, is indeed just the situation in which it has accumulated at many other points along the Delaware and its tributaries. It is probably not an exaggeration to say that dune sand occurs in greater or less quantity along the Jersey side of the river, more than half the way between Trenton and Camden, and throughout this stretch its favorite position is on the edge of the river bluff. The dune sand along the tributaries to the Delaware between Trenton and Camden occurs in the same relations. The very general presence in the region of wind-blown sand on the crests of valley bluffs leads one to suspect the same origin for sands in similar situations, such as that in which the relics are found, even when they cannot be proved to be æolian. The case is still further strengthened by the fact that human relics are very generally found in the sand which is demonstrably æolian.

In the presence of the stones there is an apparent difficulty in the way of ascribing the sands in question to the wind. If, however, the sands were accumulated by the wind after the occupation of the region by early peoples, the larger stones may have been dropped by men upon the surface at the same time with the argillite chips and half-fashioned implements, while the smaller ones might have been blown in. But we are not shut up to this conclusion. There are various other ways in which pebbles might be introduced into æolian sand. The burrowing animals and the growth and decay of the roots of trees might introduce relics and stones from the top, if they were left by men on the surface. Relics of modern civilization, bits of coal, pieces of brick, etc., were found in the sand down to a maximum depth of seventeen inches. The uprooting of considerable trees might bring up gravel stones of considerable size from depths of several feet into the surface material. If forest trees were ever upturned by winds in this locality they could not fail to bring up pebbles into the sand above the gravel. The breaks in the streaks

already referred to might find explanation in such disturbances. In view of these possibilities the presence of the pebbles in the sand cannot be asserted to prove that it is not of wind origin.[1] Finally, it is believed that no unqualified conclusion concerning the origin of the relic-bearing sand is warranted. It may be of aqueous origin, dating from the close of the last glacial epoch; it may be of aqueous origin of later age, for sea water probably covered the region at the close of the last glacial epoch or later; and it may be æolian, dating from a time long subsequent to the deposition of the sand and gravel of the plain.

Whatever its origin, it may safely be said that the surface material down to the lowest depth at which the relics have been found has been so disarranged that no affirmation can be made concerning the origin of the relics it contains. It is all within the zone of active weathering and surface disturbance. If the finds were fossils, in the usual sense of the term, it is certain that geologists would not feel warranted in attaching much importance to them.

SPECIAL EXPLORATIONS IN THE IMPLEMENT-BEARING DEPOSITS ON THE LALOR FARM, TRENTON, N. J: By Prof. G. FREDERICK WRIGHT, Oberlin College, Oberlin, O.

BY permission of Professor F. W. Putnam a number of geologists and archæologists were invited by me to conduct independent investigations on the Lalor farm in Trenton, N. J., with a view to verifying the conclusions at which he had arrived concerning the age of the artifacts found in the gravel at that place. As a result of the invitation, Mr. H. C. Mercer, Professor Arthur Hollick, Dr. H. D. Kümmel and Professor William Libbey met with me upon the ground, and we availed ourselves of the facilities afforded us by Mr. Ernest Volk, who has been for several years engaged in explorations in the vicinity of Trenton, under the direction of Professor Putnam, in the interests of the Peabody Museum, Cambridge, Mass.; the Columbian Exposition of Chicago, and the Museum of Natural History of New York City. I was upon the ground the 25th, the 26th and the 28th of June, and September 13th and 14th, 1897.[2] At this time Mr. Mercer was present every day but the 13th of September, and Professor Hollick every day but the 28th of June and the 14th of September, while Dr. Kümmel and Professor Libbey were present on one day only.

The farm on which such generous privileges have been granted by the Misses Lalor is situated on the terrace upon which the city of Trenton is

[1] My co-laborer in New Jersey, Mr. George N. Knapp, visited the locality where the relics are found, in June, and reached the conclusion that the sands in question are æolian. No one has more intimate familiarity with these sands than he.

[2] In revising proof of this paper I have included the results of the later examinations made in September.

built, about a mile below the city, overlooking the Delaware river from the edge of the terrace, which here descends abruptly to the flood-plain of the Delaware, about forty feet below. That this farm is within the limits of the so-called "Trenton gravel" is conceded by every one, and is clearly evident from a gravel pit which has been recently opened not more than three hundred yards to the north. In this pit, which has been worked to a depth of about twenty feet, the general sand and gravel are very distinctly stratified, with the lines of bedding and cross-bedding perfectly distinct up to within three or four feet of the surface. Many boulders, some of them two or three feet in diameter, occur in the lower part of the deposits. A large pile of boulders which had been thrown aside by the workmen well illustrates the observation early made by Professor Cook and Professor Shaler, that the material of the Trenton gravel is almost entirely derived from the upper part of the valley of the Delaware river, and is to that extent local material.

There is but one theory entertained by geologists at the present time concerning this gravel, which is, that it is a delta deposit made by the glacial floods which came down the river from the melting of the ice which formed the Belvidere terminal moraine, about seventy miles above Trenton. Through that distance the gradient of the river is about three and one-half feet to the mile, and the valley is narrow, so that the abundant floods of that time could easily bring down the excessive amount of *débris* released by the melting ice. On reaching tide-water and a broader valley at Trenton the swollen streams of that epoch rapidly built up the fifty-foot delta terrace upon which the city stands. This terrace is from two to three miles in diameter, upon the New Jersey side, and wherever excavated shows substantially the same phenomena described in the pit adjoining the Lalor farm, which is about the middle of the special enlargement of the deposit from north to south from which the gravel received its name.

The excavations made by our party were upon the summit of this delta deposit, beginning from the bluff where it faces the river valley to the west, and which breaks down to the level of the flood-plain, forty feet below, with as steep a descent as the gravel would naturally maintain, the slope being now covered with a luxuriant growth of forest trees.

As our investigations were made with reference to verifying the conclusions of Mr. Ernest Volk, it is proper to state the main results of his work. Mr. Volk has carefully dug over the area stretching back from this bluff for a width of about three hundred feet and a length of something like twenty-five hundred feet. Over all this area he has sunk trenches a little over three feet in depth, and carefully noted the evidences of human occupation, together with the character of the deposit and the depth at which the artificial objects have occurred. All this material gathered by him is to be found carefully labelled, but for the most part unreported upon, in the museums above referred to in our first paragraph. Mr. Volk's work has shown that the upper twelve inches of this surface contain more or less signs of vegetable mould, and a very large number of

chips and chipped implements made from flint and jasper, with occasional chipped pieces of argillite. Many other indications of the ordinary occupation by Indians also occur in this upper foot of soil; such as pieces of pottery, the bones of animals which had been used for food, and pieces of charcoal. Occasionally, also, there are pits running to a depth of two or three feet in which are similar objects down to the bottom.

But in the compact undisturbed sand from one foot below the surface through the two feet or more of the lower part of the trenches Mr. Volk reports that he has found no pottery or charcoal or chipped fragments of jasper, flint or quartz; but that he has found, sparingly scattered throughout the entire mass, many chipped fragments and implements made from argillite, these occurring frequently near the bottom of the trenches, three feet and more below the present surface and two or more feet below ordinary signs of recent disturbance.

At the first visit our own work consisted in the digging of one long trench from near the edge of the bluff back through a distance of twenty-one and one-half feet. Its width was three and three-quarters'feet, and its depth a little over three feet. This was immediately north of the ground which has been explored by Mr. Volk. We also dug three other pits, one being about twenty feet farther north, which was 7¾ x 4½ feet; another, thirty feet east of the second, which was six feet square; a fourth, one hundred feet still farther east, and on ground about one foot higher, which was four and a half feet square.

After careful examination we fixed upon fifteen inches as the limit of probable or possible ordinary disturbance, and had all the superincumbent soil removed from the trenches to that depth. We then had narrow excavations made two feet lower or to a total depth of forty inches, in some cases going still farther down. Having prepared a smooth perpendicular surface, the work was subsequently done by carefully scraping off the face of the excavation with a trowel, and when any object of stone was encountered, all were called to witness it in place before removal. In this way a total amount of three hundred and twenty-five cubic feet of the deposit was carefully examined. This is equivalent to a trench forty feet long, four feet wide and two feet deep; this being the lower two or more feet of the material removed.

Number of objects found.—As a result of this examination during the first visit, there were found in the lower two feet of undisturbed soil fifteen chipped fragments or implements of argillite all covered with a deep patina, two thick flakes of jasper and three of quartz, with a few broken stones. There were found and counted also between three hundred and four hundred pebbles ranging from the largest, which was 8½ x 4¾ x 2½ inches, down to numerous ones the size of a French pea. Many of them were from one to two inches in diameter. The implements were scattered pretty evenly through the entire mass of gravel. The upper five inches contained four; the second five, seven; the third five, four; the fourth five, five. Besides this there were six battered or broken pebbles, showing artificial origin, but all these were in the upper six

inches of our stratum. Of the quartz and flint flakes, two were found in the upper four inches, two from ten to twelve inches down, and one at a depth of eighteen inches.

The character of the strata.—Beginning at the top, there is first a stratum of fifteen inches to be excluded. The upper seven inches of the strata under consideration consists of compact sand in which there are no signs of bedding or disburbance of any sort.

Below this occurs, over most of the area, a reddish, clayey band, about a half-inch thick, which extends in a wavy line, often thinning out so as to be almost imperceptible.

Below this is a stratum of yellowish sand, similar to the first, about six inches thick.

Below this is a second continuous reddish band slightly waved, but far less so than No. 1, and about two inches thick, thinning out, however, in places to half an inch or slightly less.

Then occurs another stratum of yellowish sand, about four inches thick.

Still lower comes reddish clay,[1] ten inches thick, partly divided by an irregular stratum of sand, two or three inches thick, running through the middle of it. In some places, however, the clay bands coalesce, and the sand continues along the line in lenticular masses. This clay stratum rests upon the more completely washed sand and gravels which form the great mass of the terrace.

Analysis of these strata.—The predominant material in the entire thickness examined is sand, but there is mixed with it a varying amount of clay and iron, both being specially concentrated in the red bands. I have had specimens from these strata analyzed to determine the relative amount of clay, sand and iron contained in them. The specimens were first thoroughly and repeatedly soaked and shaken in water, which was poured off immediately, carrying everything which could be held in suspension, and leaving the sand thoroughly washed. After being allowed to settle twenty-four hours, the water was siphoned off and the residue thoroughly dried. This was then compared in bulk with the residue of sand which had also been dried. All the specimens were treated in a similar manner. The results were, beginning at the top:

1st sand stratum, 18% suspended matter; 2d sand, 16%; 3d sand, 14%; underlying sand, 7%. Of the red bands, 1st red stratum, 24% suspended matter; 2d red stratum, 27%; 3d stratum, 27%; 4th (or lower part of 3d) stratum, 37%. The amount of iron contained in the fine sediments so far as analyzed was as follows: from the 2d sand, one-half of one per cent of the whole amount; of the 2d red stratum, one and one-half per cent of the whole amount, and the same for the 4th red stratum. These red bands, therefore, are not to any appreciable extent segregations of iron. The iron scarcely more than suffices to give the color. The analysis shows, therefore, that these red bands contain from 25% to 33% more clay than is found in the interstratified strata of sand.

[1] Throughout I use clay as signifying sediment so fine that it is held in suspension in water for an appreciable time.

While the water-worn pebbles were distributed more or less through the entire mass, they are specially abundant in and below the second, third and fourth reddish clayey accumulations. These pebbles vary in size, as has been said, from those several inches in diameter down to small pieces of gravel. Pebbles from one to two inches are numerous. The largest one referred to was imbedded in the clayey stratum, No. 2, just as if dropped into the clay and pressed nearly through it, crowding lower down two or three smaller ones which underlay it. This was found in the longer trench, seventeen inches below our selected zone of doubt. This was about three feet distant from the locality of one of our best-formed chipped argillite scrapers, which occurred three inches lower down in the deposit, and beneath the clayey stratum. As I personally removed with my trowel all material in this section, I know that the clayey stratum containing the large pebble was continuous and unbroken, not only between the pebble and the point directly over the argillite implement, but for two or three feet upon the other side, and that pebbles, large and small, were frequently struck by the trowel along the entire distance.

The implement above referred to was located carefully and photographed by us. The first clayey band over it was here about two inches thick and so compact as to give distinct special resistance to the trowel wherever it was struck. Its level was twenty inches below our zone of doubt, or thirty-five inches below the surface. Upon our last visit, however, two well-formed argillite implements were found at still greater depth, and if possible in still more unequivocal position. One of these, found by Professor Hollick, was in the main trench more than twenty feet farther east. This was, also, below the second clayey band, which is here nearly ten inches thick. The implement was three feet five inches below the surface, or twenty-six inches below the arbitrary line we had fixed as our zone of doubt. The order of strata was as follows: one foot, disturbed soil; eighteen inches, compact yellow sand; first wavey reddish clayey band, one-half inch; yellow sand, two inches; second reddish clayey stratum, with inclosed lenticular sand patches, ten inches; sandy layer, five inches, containing the implement; reddish clayey band, five inches. Below this, without any signs of unconformity, were several successive alternating strata of sand and reddish clayey material to the total depth dug, six feet nine inches. There was every indication that the entire deposit up at least to the upper wavy reddish clayey stratum was built up by a continuous process of water deposition, the upper portion representing the waning cycles of deposition characteristic of floods which were reaching their limit both by reason of increased elevation of the flood-plain and of a diminution of the water supply.

Eight feet north of this implement, across the end of the trench, with the strata continuous between, Mr. Volk had previously photographed another argillite implement in place one inch lower than this and beneath almost exactly the same succession of strata. Again, on the face of one of our earlier pits farthest from the bank, about forty feet distant from

this one, we found on the second visit and photographed, and Mr. Mercer took out with his own hands, a well-formed argillite implement three feet three inches below the surface, or two feet below our arbitrarily chosen line of doubt. There were two of the clayey bands above this, the order being closely similar to that described in the trench. The second of the clayey bands, which is here about two inches thick, contained a pebble more than two inches in diameter, which was pressed down into the stratum as if by its own and the superincumbent weight. On removing the pebble the clay adhered to it, but was not cemented to it. It could all easily be removed with the fingers. Two or three feet east of this implement were the marks of the tap-root of a tree which had penetrated all the strata to a depth of something over two feet. It was perfectly easy to distinguish the course of this root, but there were no other signs of such disturbance around the face of the exposure in the vicinity of any of the implements described.

Extent of the reddish clayey bands.—To determine the extent of these deposits and the manner of their formation, pits were dug at three points, from two hundred to five hundred feet farther in from the edge of the bluff. Two of these were upon a slightly elevated ridge, from two to three feet higher than our main trench, being the highest portion of the plain. The other was in the center of a shallow basin, towards which there was a gradual slope in every direction, it being from two to three feet lower than the trench and about two hundred feet away. In all these pits the succession of strata was similar to those in the main trench, with this difference, however, that the reddish clayey bands were encountered several inches lower down in the center of the basinlike depression than upon the higher elevations. This effectually disposes of the theory that the slight irregularities in the surface were produced by wind action. If the elevations were wind accumulations the sand would be thicker there than in the depressions, but the opposite was the case. The slight irregularities of the surface, therefore, are the result of original water deposition and not of wind action.

Observations upon other portions of the delta terrace revealed the same condition of things. In the gravel pit already referred to, about three hundred yards to the north, the succession in the upper three or four feet exposed is substantially the same. The clayey bands are distinctly apparent, and the whole structure is conformable. In this pit there occurs, and was photographed by us, a thick deposit of fine sand near the bottom, containing thin wavy bands of stratified clay, very similar to the upper one described in our main trench. Again, about a mile distant, toward the southeast, near the park, an excellent exposure of the terrace shows the same succession. With this, also, agrees the description given by Mr. Volk of his explorations on the same terrace in 1891, at a point about half way between the park and the present explorations. (See Proceedings of the A. A. A. S., Vol. XLII, 1894.) In describing the strata, he says: "The soil in this place consists of nine inches of black or subsoil, tilled land, overlying an undisturbed sandy loam composed of

quartz sand, colored by iron and mixed with a yellow soil, the sediment of muddy water, the whole having a light yellow color. Three feet below the surface is a somewhat uneven stratum of red clay mixed with sand."

From all this it is clear that this deposit, up at any rate to the upper line of reddish clayey band, is part and parcel of the Trenton gravel.

A most natural explanation of the deposition of these successive strata of sand and clayey bands is at hand in the closing floods of the glacial period which confessedly built up the terrace to within a few inches of several of the implements found in place. All the phenomena can easily be accounted for by the forces then known to be in operation. The floods accompanying the close of that stage of the glacial period which formed the terminal moraine crossing the river at Belvidere brought down the *débris* from the melting ice until the delta terrace was built up to a height of about forty-seven feet. This, all readily admit. But it is easy to see that as the delta grew higher, and the material accessible to floods diminished, the closing deposits would consist of finer material, the conditions being somewhat like those of ordinary flood-plains, only in this case the forces were more extraordinarily variable and vast in their proportions. In the last stages of this epoch we may well suppose that during the months of July, August and September the waters running over this delta terrace were occasionally swollen enormously, though the elevation overflowed was such that any large masses of boulder-laden ice were prevented from sweeping over it, such as did during the earlier stages of the deposit. These floods would easily distribute large quantities of sand along the edge of the terrace of the main stream, extending back for an indefinite distance or to the slightly higher deposits that had previously been made, and this under conditions so uniform that several inches might be accumulated without signs of bedding. On the subsidence of the floods the clayey strata would naturally accumulate, as shown in these deposits.

But how did the implements become incorporated in the strata? By a process which is perfectly natural and credible. During the larger part of the year, when the melting of the glacial ice was proceeding at a slow rate, vast bars and abandoned channels of the main pebbly deposits of sand and gravel would be exposed, affording to the aboriginal inhabitants a choice field for procuring argillite boulders, with an occasional one of quartz or jasper, from which to make their favorite implements; while the conditions connected with the head of tide-water doubtless made it then, as later, a favorite hunting and fishing ground. Few things have ever impressed me so much as the abundance of life of all sorts at the head of the inlets, both in Alaska and Greenland, into which large glaciers pour their currents of ice-cold water. The implements and chips lost by the natives on these temporarily abandoned stretches of gravel and sand bordering the main current are the ones mingled with scattering pebbles which were swept into their present position upon the Lalor farm by the subsequent floods of the season.

But this condition of things did not remain long. After the flooded Delaware ceased to receive superabundant glacial *débris* and an excessive supply of water from the melting ice it not only ceased to build up the terrace, but began speedily to cut and enlarge its present channel, leaving the surface of the delta terrace forever after undisturbed by its action. The terrace is now about forty feet above the flood-plain. Thus we have a natural and perfectly credible method for accounting for the phenomena in question.

It is, however, not only incumbent to provide an adequate cause for such phenomena, but we are in duty bound to give sound reasons for excluding other hypotheses which may be supposed to account for the facts. One hypothesis is that the clay band No. 2 has been produced by segregation, and so may have been formed over the implements found underneath it, since their deposition. But the stratum contains only a slight amount of iron in excess of that found throughout the entire sand deposit. There still remains the excessive amount of clay which characterizes the stratum. It can scarcely be possible that that was segregated over so extensive an area after the whole had been deposited. Besides there must be something to account for the slight excess of iron which characterizes the red stratum. It is not only slightly excessive in amount above what is found in the accompanying sand, but is different in color; that being yellow and this red.

The suggestion that this clayey stratum No. 2 is an old wind-blown surface encounters several insuperable objections.

1. Its extent and uniformity are greater than could be obtained by a wind accumulation.

2. It contains large numbers of pebbles too great for removal by winds. As already mentioned, one of these was several inches in its longest diameter, and many were over an inch in diameter. To the suggestion that these may have been brought upon the surface by human agencies at the same time that the chipped flakes were lost, it can be answered that many of these were too small to have offered any inducement to anybody to have brought them up into that place, and they are scattered through the formation so uniformly that they indicate distribution by natural agencies; while their occurrence in the clayey stratum points to water, and not to wind, for their distribution. Especially significant were the two or three small pebbles underneath the large one which lay as if pressed by the weight of the larger pebble into and almost through the clayey stratum.

To the suggestion that these pebbles had been brought up from the lower strata by overturning of trees, it is to be said that if that were the case it must have been before the formation of the clayey stratum, since that by actual observation was continuous and undisturbed for a distance of many feet on either side of many of these pebbles.

To the theory which would account for the iron in the stratum through the oxidation of the vegetable accumulation occurring upon the surface, it is to be replied that this would not account for the accumulation of

clay, but rather it needs the accumulation of clay to arrest the soluble iron in its downward progress. Whatever may be true of that upper and more wavy film of reddish material, it cannot well be maintained that stratum No. 2 is of a different origin from that of the thicker strata 3 and 4, the last of which rests directly on the acknowledged glacial gravel. In case of these it cannot be maintained that they are the results of a progressive oxidation proceeding from the surface downwards, since they are distinctly separated by the strata of yellow sand. There must have been something in the original deposition to have determined the relation of these differently colored strata to each other, and that original stratification has not been to any great extent disturbed by subsequent agencies.

If these red bands are the results of oxidizing agencies connected with vegetable deposits, they must still be placed long anterior to the accumulation of vegetable mould which is now upon the surface; for in many cases Indian pits are found in which the vegetable mould is entirely unaffected by oxidizing agencies; and, as this farm is known to have been occupied by the whites two hundred years, we have a partial measure of the slowness with which vegetable matter decomposes in this soil.

Furthermore, if the action of tree roots, worms and other small animals were so great in breaking up and destroying the lines of stratification two or three feet below the surface, as some have supposed, these clayey strata must be very recent, else they would have been completely obliterated. But their integrity is a complete answer to the theory that the soil down to a depth of four feet is everywhere necessarily disturbed through the lapse of long periods of time. In the present case every attempt to account for the clayey strata 2, 3 and 4 otherwise than by the agency of water is beset with insuperable difficulties. There are none of the marks of those other supposed agencies left to indicate their activity. Whereas, with the acknowledged floods of water rising to within a few inches of these deposits, it is easy to believe in the extension of this agency in building up the deposits of the superincumbent fifteen inches. If any one denies this natural and easy explanation he is bound to bring forth reasonable evidence to the contrary. To adduce complicated hypotheses involving inadequate and unknown causes, of which there are no signs present, is not sufficient.

The evidence that the implements found below stratum No. 2, forty-one inches below the present surface, and only five inches above the action of acknowledged glacial floods, belong to the deposits of the glacial floods, is sufficient, I believe, to convince any one who comprehends all the facts. At any rate, it is in the highest degree hazardous to assert that the problem of their age is insoluble, and that no trustworthy inference concerning it is possible. For a short time the facts can remain open for the observation of others. Let any one who is in doubt visit the locality and see for himself.

Professor Libbey of Princeton University spent the afternoon of September 14th with us on the ground when all the exposures were open for

inspection. These he photographed and carefully examined with reference to all the questions involved. In respect to them he writes as follows: "Princeton, September 20, 1897. I take pleasure in putting on record my opinion with reference to the deposits which we studied together last week. There is no doubt in my mind as to the origin of the deposits in which the various implements were found. The lower gravel layer immediately below is undoubtedly due to water action, and I cannot believe that the banded region composed of sand heavily mixed with clay could have been laid down in any other way than those just below. Certainly no wind action can ever be held responsible for such level and uniform deposits over such an area, but it might rather be explained by a change in level affecting the velocity of the current — in fact, I believe the existence of this bed can be explained only in this way."

PRIMITIVE MAN IN THE DELAWARE VALLEY. By Prof. W. H. HOLMES, U. S. National Museum, Washington, D. C.

INTRODUCTORY.

A FEW years ago, as a result of extended explorations, conducted by the Bureau of American Ethnology, questions were raised with reference to the soundness of the then existing evidence relating to glacial man in the Eastern States, and the correctness of the conclusions drawn from it. Since that time, until quite recently, investigation has progressed slowly and but little has been brought forward likely to change the status of the case. Now, however, strong claims are being made of the discovery of new and confirmatory evidence of antiquity, and discussion is invited with a view of determining its merits; but before taking up this phase of the subject it is desirable that the earlier phases of the investigations be passed briefly in review.

The questions raised by me were not those of the age of man in America. I have always taken the view that the race must have occupied this continent for a very long period. Great antiquity is clearly proved by facts derived from other than archæologic or geologic sources. It does not require argument to show that the development of many well differentiated nations and tongues means a prolonged occupation. It does not take argument to demonstrate the proposition that, notwithstanding the potent influence of local environment upon human art and effort, a thousand distinct cultures could not spring up in a day.

The only questions I have ventured to discuss and the only ones that now claim my attention are as to whether the evidence already brought forward to demonstrate the antiquity of man on the Atlantic slope will stand the test of scientific scrutiny. There is a record of man in the valleys and among the hills throughout the entire country. There is an important record in the geological formations of the Delaware valley.

Has the key to this record been discovered? Has the true combination been worked out, or are our pioneer investigators struggling through a phase of this particular research corresponding to that encountered by the predecessors of Champollion in the reading of the Egyptian hieroglyphs? The earlier readings at Trenton seem to indicate possibly three distinct peoples and periods of occupation, referred to by some as paleolithic, Eskimo and Indian; but are we sure of more than one and are the others mere figments of the imagination? Time will tell, but this year or the next may not finally decide it.

THE ALGONQUIAN OCCUPATION.

The first step in acquiring a knowledge of the past is to seek to understand the present. An acquaintance with the historic peoples of a region is the best key to the prehistoric peoples. In the study of the question at issue in the Delaware valley correct method demands that we look first to known conditions for explanations of all doubtful phenomena. The only occupants of this region known to us were a group of Indian tribes of what has come to be known as the Algonquian stock. The history of these tribes, as dimly shadowed forth by tradition and archæology, extends back indefinitely into the past. They were found by the whites living in villages, cultivating corn, navigating the waters, hunting, fishing and warring; weaving simple fabrics, practising the potter's art in its most primitive form, and employing stone as the chief material for implements and weapons. They used metal to a very limited extent and employed shell, bone and wood in various arts. Their culture status is made clear by actual observation of the peoples themselves, as well as by a study of the relics of many village sites known to have been occupied by them. The local tribes, the Leni Lenape, had relatives of like culture extending along the coast from Carolina to Maine and from the mouth of the St. Lawrence to the head of the Great Lakes. They had neighbors of other stocks, all occupying about the same simple level of neolithic culture. Researches long continued in the whole vast territory occupied have developed no definite trace of other people or other conditions of culture. No one can say how long they had been here or whence they came, but their coming was doubtless long ago. Wandering bands pushed their way over the hills or along the shores and gradually took possession of this beautiful region. One group, known to us as the Delawares, occupied the Delaware valley, adopting it as a permanent home. Their dwellings were established along the banks of the rivers and creeks; they multiplied and spread, and, being an active and enterprising race, gradually acquired a knowledge of the resources of the country, and especially of the varied mineral products, which were of the utmost importance to their welfare. On local sites they worked the varieties of stone available for implements. They dug them out of the loose deposits of the stream beds and bluffs. They advanced into the hills and mountains, and little by little discovered the deposits of desirable rock in place, and quarried

deeply into the bowels of the earth. The work of search and exploration was so thorough that nothing escaped them, and the archæologist looks with amazement on the still existing evidences of their energy in quarrying argillite, jasper and soapstone.

The stones available to such a people in the earlier periods of their occupation would be the loose cobbles and masses of the rivers and bluffs. In the Trenton region the only material well fitted for flaking — the chief shaping process of the early days — was argillite, a compact slaty-looking rock especially plentiful in some parts of the glacial gravels. It follows that on and about the margins of the glacial terraces flaking at first dealt chiefly with this material. The beds of argillite found in place farther up the valley would next be utilized and later the flints and jaspers of the distant uplands would be discovered and used. How long it was from the time of the first occupation to the period of complete exploration and utilization of resources thus outlined no one can guess. It may have been five hundred or it may have been five thousand years. During this prolonged period the work of shaping stone implements went on. The raw material was sought and worked up with a persistence and energy that might almost be regarded as a foreshadowing of the vast mining and manufacturing industries of to-day. The knives, scrapers, drills, projectile points, etc., the implements upon which everything in the savage economy depend, were roughed out, specialized and carried away, and the refuse in vast quantities, consisting of flakes, fragments and failures representing all stages of development, was left upon the ground. The rejectage must have been especially plentiful along the bluffs at Trenton, where the argillite was found in the shape of bowlders and partially worn masses, and in the valleys and hills above, where it occurs in place. The rude rejected forms left upon these sites were large or small, long or short, according to the shape of the implement made and the nature of the material used. They were rough or well developed according to the stage of the shaping process at which they were cast aside. No type of flaked stone has been found in the whole region that was not necessarily produced again and again and for centuries along the banks and bluffs of the Delaware by these historic peoples, and in the course of years and geologic mutation it is readily seen that this rejectage of implement-making would become intermingled in various ways with the superficial deposits of the sites of manufacture. Every bank that crumbled, every grave dug, every palisade planted, every burrow made, every root that penetrated and every storm that raged took part in the work of intermingling and burial; and following in turn came the resettling, the leeching-out and the recementing of these deposits, making it difficult to distinguish the old from the new. It follows, therefore, that the student of the history of this valley, and especially of that part of it recorded in the soil and superficial deposits, should not for a moment lose sight of these conditions and events of recent and comparatively recent history, and should seek first to explain all phenomena from the point of view thus afforded before conjuring up shadowy images of other races.

INVESTIGATION IN THE GLACIAL GRAVELS PROPER.

It happened, however, that before the investigation of the phenomena referred to above and now so definitely assigned to the Algonquian peoples had begun to attract the attention of archæologists the presence of other people had already been assumed. Evidences of very primitive paleolithic races had been associated with glacial formations abroad, and the glacial deposits of the Delaware region were accordingly searched with the hope of finding similar traces. Relics of art were soon secured, and as they were rude and exclusively of flaked stone they were regarded as supporting the theory of a glacial paleolithic man. A large body of evidence was soon accumulated and passed into literature without particular scrutiny.

When, finally, this subject came into prominence and questions began to arise as to the determinations made, it was found that the flaked stones which formed the exclusive evidence furnished, though rude as reported, were not of special or peculiar types, such as seem to characterize paleolithic times abroad, but that they corresponded in every particular with the ordinary rude work, and especially with the rejectage of manufacture, of the Algonquian and other American tribes; and it happened further that they were found along the very bluff faces where argillite bowlders outcropped and where the Indian tribes had naturally resorted to secure the raw material and block out their implements; then it came to be asked whether the finds had really been made in the true gravels, whether they were not obtained from deposits associated with the gravels but not belonging, in their present deposition, to the glacial age; such deposits as accumulate in depressions, or along the faces of bluffs and banks subject to crumbling and sliding. When later it was realized that the questions involved, the nice discriminations to be made in collecting this evidence, were really geologic rather than archæologic a new phase of the investigation was initiated and geologists were asked to participate in the examinations. It was imperatively demanded that the gravel should be reëxamined and the evidence sifted and placed on a safe footing. In meeting this demand for reëxamination of their evidence, the advocates of an American paleolithic man have claimed that the criticisms made were to be classed with those encountered by Boucher de Perthes when he began to present his evidence regarding early man in Europe; that such criticisms meet every advance of thought. But the cases are by no means parallel. The discoveries of Boucher were not acceptable because of their revolutionary character with respect to accepted beliefs. On the other hand, the Trenton determinations were popular and almost universally accepted as final until attention was called to the true nature of the objects found, and especially to the unsatisfactory methods pursued in collecting evidence. The climax came when it was understood that the advocates of a glacial paleolithic man were gathering all classes of rudely flaked stones from the surface of the country generally (entirely disregarding an Indian occupation) and employing them to establish a peculiar theoretic culture for America.

It was not conservatism, and especially it was not conservatism in religious thought, that led such men as Powell, Brinton, McGee, Chamberlin, Salisbury, Mercer, Mason, and others intimately acquainted with the field of investigation seriously to question the methods and the evidence. The charge of conservatism must rather be urged against those students who have held to their original views and especially against such scholars as Topinard, Boulé and Keane, who accept without serious scrutiny any evidence that tends to confirm accepted theories with respect to a uniform history of the race on both sides of the Atlantic.

Fortunately opportunities for a reëxamination of the evidence have arisen in several cases. The principal discoveries of shaped stones attributed to the gravels were made in the slope of the bluff facing the river at Trenton (*A* in the section, Fig. 1) and in the banks of Assanpink Creek at the point where the Pennsylvania railway makes its way from the station near the creek level to the terrace above (*B* in the section). Finds continued to be made in the crumbling river bank at *A* until accumulating city refuse covered up the deposits.

They ceased to be made in the creek banks at *B* as soon as the cutting extended fairly into the gravels in place; and when, in 1889, I asked the principal explorer of this locality why the finds had ceased, he replied that when the railway cutting was made the excavations were carried up through a depression that must have been an old stream bed, and that the finds were in the filling of this channel. I do not think he understood the significance of the admission, but the statement must have been true, as nothing whatever is to be found in the present excellent exposures of the true gravels. The position of his finds is indicated at *d* on the dotted original profile in the section, and the present utterly barren exposures, half a mile or more in length, are indicated at *e*.

Identical results have been reached on the river front *A*. In 1892 a great sewer trench, *C*, 33 feet deep, was cut, parallel with the river bank, at the very point where so many shaped stones had formerly been found. Though we kept up the search in this trench for five weeks as the work of excavation went on — the whole body of gravel being subjected to rigid examination — not a chip was found, not a trace of man. No other examination has been made that compares with this for thoroughness and length of time involved. The evidence thus furnished has been spoken of as negative and hence as unsatisfactory, but, in the continued absence of finds of implements at this and other points, it seems positive and convincing. The conclusion reached is that there must have been an error in the observations that could produce hundreds of flaked stones from obscure or partial outcrops at a given spot in a crumbling bank when not a trace can be found at the same point when the beds are fully exposed.

Geologists will be interested in seeing the detailed section (Fig. 2) made by my assistant in the trench. It tells the story of the deposits better than any other section that has been or probably ever will be made.

Considering the foregoing facts, it may be regarded as substantially proved that the glacial gravels proper contain no relics of art, and it

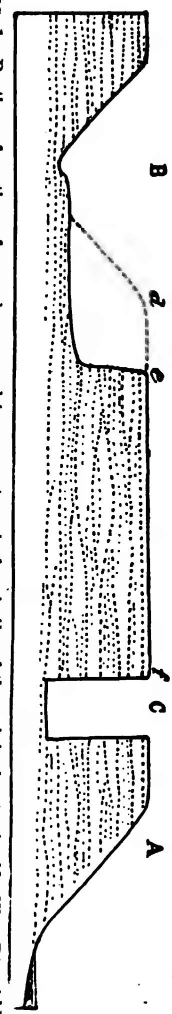


FIG. 1. Portion of section of gravels exposed in sewer trench, fac-simile of the original notes by Mr. Wm. Dinwiddie.

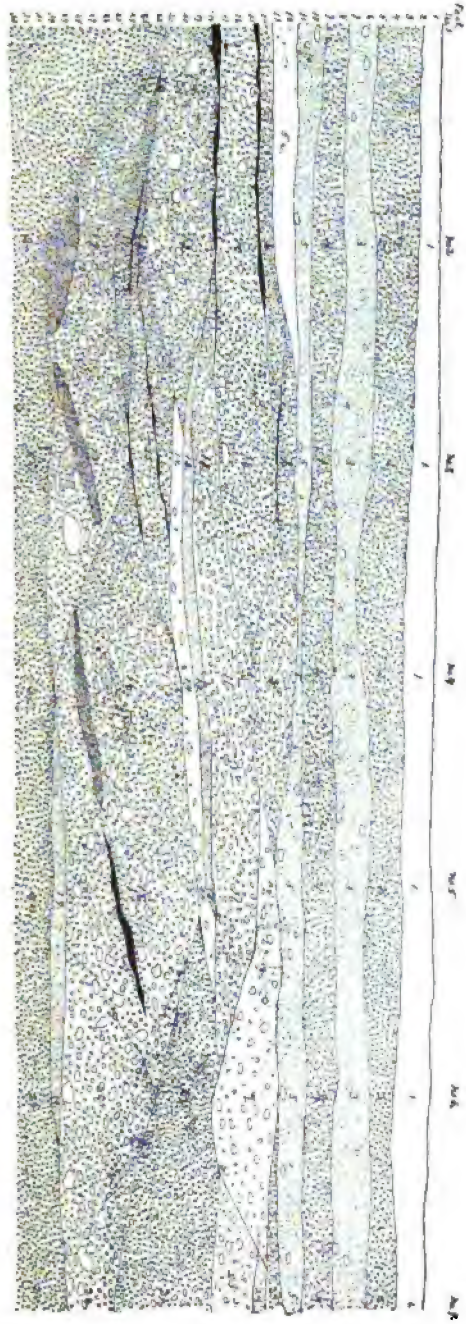


FIG. 2. Section of the Trenton gravels showing relation of productive to non-productive profiles.

would appear that now very few persons, indeed, expect them to yield any evidence whatever on the subject of human occupation. Five years have passed since the earlier observations and finds were challenged, and in that time, so far as I have learned, no single implement has been reported from the gravels, although the exposures are as extensive as they ever were. The first chapter in the prolonged search for glacial man at Trenton may, therefore, be regarded as practically closed; but some new evidence furnished by examination of certain superficial deposits of sand come up for consideration. My remarks upon this subject will appear in a future number of Science.

A NEW INVESTIGATION OF MAN'S ANTIQUITY AT TRENTON. By H. C. MERCER, Doylestown, Pa.

A RENEWED search for evidence of glacial man at Trenton induced Professors G. F. Wright and Arthur Hollick, Messrs. H. B. Kümmell and G. N. Knapp, of the New Jersey Geological Survey, and myself, on June 25, 26, 27 and 28, 1897, to explore a site on the summit of the glacial gravel terrace at Trenton, where Mr. Ernest Volk[1] has alleged the discovery of artificial argillite flakes, and other relics of human handiwork, in a yellow sand of supposed glacial age under a surface loam stratum indicating Indian occupancy.

The interesting site, a study of which might quickly decide the much vexed question of man's antiquity in Eastern North America, occupied a part of the cultivated farmland upon the extended summit of a bluff on the left Delaware bank, about a mile below the city limits of Trenton. Within the reserve[2] of a large cornfield, we stood on the brink of a comparatively level area of ancient river washed gravel, produced, according to geologists, by the depositing agency of extensive freshets in the Delaware caused by the melting of the great glacier from ten to thirty thousand years ago. At a height of about fifty feet above the common water level we looked around a projecting promontory of the terrace, at the river, which, distant about one mile to the southeastward, wound through the swampy bottoms below. These lowlands extending from the margin of the stream to the base of the bluff and, widening at this point where the latter receded from the river, were subject to frequent inundations; but our foothold on the bluff top, though itself originally constructed by the river, was now out of reach of the highest freshet by about thirty-five feet.

[1] Working under the direction of Professor F. W. Putnam for the New York Museum of Natural History, Mr. Ernest Volk was generally present at the digging and hospitably offered us every assistance, being seconded by the kindness of the owners of the property, the Misses Lalor, who generously placed their land at our disposal. Dr. Charles C. Abbott was continually present, and Dr. Harrison Allen, of the Academy of Natural Sciences, of Philadelphia, visited the trenches on Sunday, June 27th.

[2] Kindly reserved for our use by the Misses Lalor, owners of the property, for whose generous hospitality we herewith return our thanks.

Fig. 1.

Fig. 1. — View of face of Trench A, showing Argillite specimen (see middle right-hand specimen in Fig. 3, Professor Hollick's paper) resting in place, 20 inches down, in the yellow sand at base of the rule. The trowel scratch marks the position of the film of stratification just above the specimen. The lower scratches mark the thickening of these films at the base of the yellow sand From surface to trowel, Layer 1 (Indian), ten inches to one foot. From trowel to series of scratches, Layer 2, yellow sand, relic bearing, eighteen inches to two feet Below scratches coarse sand and gravel, non-relic bearing. Specimen photographed in place June 26, 1897, by Ernest Volk, in presence of G. F. Wright, H. B. Kümmel, C. C. Abbott and H. C. Mercer.

Whatever objects therefore might be discovered by excavation on the high level of the bluff top, after penetrating its superficially disturbed soils, might be suspected to pertain to the glacial bedding of the site, and to partake therefore of the antiquity of the latter.

With much interest in view of the discoveries of Mr. Volk, our party caused trenches to be dug at various points at the spot in question over an area 90 feet long, and 33 feet broad.[1]

The excavations, exhibiting against their freshly dug sides, the upper construction of the bluff, revealed clearly the following layers:—

Layer 1.

(10 inches to 1 foot.)

A discolored surface loam, disturbed by recent cultivation, mixed with the remains of the Indian and white man. It contained:

	Specimens.
Blocked-out blades, argillite	3
Blocked-out blades, chert	2
Argillite blade, long, narrow (spear form)	1
Arrowhead, broken, jasper	2
Arrowhead, broken, chert	1
Hammerstone, pebble, fragment	1
Animal bones, with large scapula, probably Elk splintered limb bones and bone of a bird	22
Mussel shell (Unio) fragments	6
Pebbles, probably fire-cracked	131
Teshoa, or pebble flake, scraper?	1
Argillite flakes, probably artificial, generally with green patina	19
Chert flakes, probably artificial	11
Jasper flakes, probably artificial	5
Stone (unidentified) flakes, probably artificial	6
Quartz and quartzite fragments	5
Potsherds, Indian (8 found together)	9
Pebbles unworked; size, from that of robin's egg to goose egg; largest 4 in. in diameter	55
Miscellaneous stones, mostly pebble fragments	103
Oyster shell	1
Cinder fragment	1
Anthracite coal fragments	2
Glazed potsherds	2
Brick fragment	1

The objects thus disclosed sufficiently characterized Layer 1 as a layer representing a site of Indian occupancy, one of the familiar superficial riverside towns or camps of the North American aborigines. As usual at

[1](Trench A, 21½ ft. by 3¾ ft. by 3 ft. 3 in. deep; B, 7¾ ft. by 4½ ft. by 3 ft. 3 in. deep; C, 6 ft. by 6 ft. by 2 ft. 8 in. deep; D, 4½ ft. by 4½ ft. by 3 ft. 3 in. deep, well upon the flat summit of the terrace, with Trench D, about 80 ft. from the outer brink.)

such spots objects of white man's make. or use such as glazed potsherds and fragments of brick and cinder had become plowed into or otherwise mixed with the blackened earth and Indian rubbish. This latter consisted of the usual worked pebbles, mussel shells, arrowheads, blades and chips of chert, argillite and jasper, Indian potsherds and animal and bird bones (rare through decomposition at other riparian sites) buried at the usual depth, mixed in the usual proportion, and showing the usual amount of decomposition. The mixture was indistinguishable from the refuse characteristic of other village sites on the river, such as those examined by me at Fry's Run, Gallow's Run, Ridge's Island and Lower Black's Eddy. One thing was clear, the riparian Indian, presumably the Lenni Lenape, or their kin, last owners of the soil, had for a considerable time before their expulsion from the valley in 1737 made use of the bluff top as a camping site.

But the zone of interest lay below this rubbish. Under Layer 1, and as the chief centre of observation, rested

Layer 2.

(18 inches to 2 feet.)

It was composed of a fine yellow sand (easily distinguished from the dark color of Layer 1 above it), streaked towards its lower portion with fine irregular films of reddish sandy clay which thickened and coalesced at the bottom into a distinct harder red band.

Advancing slowly into Layer 2 with trowels against the sides of the trenches where the stratification had been exposed in vertical section, we pulled out one by one the pebbles, broken stones and artificial chips, etc., described below; giving special attention to the large pebbles and the chips most artificial in appearance, calling the attention of all our colleagues to each large object, when it first clinked against the trowel's edge, so that its depth could be measured and its position with reference to the clay films, observed by us all before its removal.

The objects thus found (after discarding those resting in the upper part of the layer for about four inches below the bottom of *Layer 1* as pertaining to what we called the zone of doubt) were as follows:

ARTIFICIAL FLAKES.

NOTE.—The numbers in inches after the specimens indicate the depth in Layer 2 below the zone of doubt, the letters the trench referred to.

	Number of Specimens.
Jasper, 10 in.	1
Jasper, 12 in.	1
Quartz, 4 in.	1
Argillite green patina, 6½ in.	1
Sandstone or argillite, 2½ in.	1
Argillite lying flat, 3¾ in.	1

Fig. 2.

Fig. 2. Artificial chert specimen (see also upper right-hand specimen, Fig. 3), photographed as found in place in the yellow sand. The trowel scratches show the line of films of stratification above and below the object. The dark, deep scratch marks bottom of Layer 1 (Indian), 10 inches to 1 foot. Therefrom the yellow sand, Layer 2, extends downward. Photographed by Mr. Volk in the presence of Professor Wright, Dr. Hollick and Mr. Mercer.

	Number of Specimens.
Argillite, 7 in.	1
Jasper, 12 in.	1
Argillite, 20 in. specimen $1\frac{5}{8}$ in. wide, $\frac{1}{8}$ in. thick, Trench A 2 in. resting on its side below a thin film of reddish clay. The film waved from one side of the trench to the other over the specimen, becoming slightly thinner and fainter directly above it, though without showing signs of break. Photographed in place 20 in. See Figs. 1 and 3	1
Argillite green patina, about 1 in. in diameter, possibly artificial, resting exactly in line of clay film, 12 in. . .	1
Quartzite, possibly artificial, 8 in.	2
Chert (resting indubitably in films of sandy clay. Trench B. Photographed in place, signed G. F. Wright, Arthur Hollick, H. C. Mercer). See Figs. 2 and 3 (in Dr. Hollick's paper), 12 in.	1
Flakes, argillite, small below clay film, Trench B, 14 in. .	1
Quartz, Trench B, 8 in.	2
Quartzite pebble flake, 10 in.	1
Argillite, possibly artificial, C, 12 in. . . .	1
Quartzite, possibly artificial, C, 10 in. . . .	1
Jasper, possibly artificial, C, 6 in.	1
Argillite, possibly artificial, C, very small piece, $2\frac{1}{2}$ in. .	1
Chert from pebble, 6 in.	1
Argillite, possibly artificial, 3 in. long by $1\frac{3}{4}$ wide, green patina under a very distinct film of reddish sandy clay, Trench C, 19 in.	1
Argillite, possibly artificial, exactly in line of heavy red film and 6 in. below fainter film, C, 16 in. . . .	1
Quartz possibly, D, 6 in.	1
Chert from pebble, 6 in.	1
Argillite, probably artificial, bedded in filmings of reddish sandy clay not immediately overlaid by a continuous film, but rather by faint reddish broken streaks, D, 15 in. .	1
Argillite patinated, $2\frac{3}{4}$ in. long, $1\frac{1}{2}$ in. wide, $1\frac{1}{8}$ in. thick, probably artificial, resting in series of clay films less clearly marked than in the other trenches, but nevertheless extending over the specimen in a series of blotches, D, 16 in. .	1
Argillite piece, possibly artificial, on film, 15 in. . .	1
Argillite piece, possibly artificial, in C, 12 in. . .	1
Total	30

ANTHRACITE COAL.

Small fragment size of hazelnut, below zone of doubt, in yellow sand	
Anthracite coal, small piece size of hazelnut, C, $3\frac{1}{2}$ in.	
Total	2

ROOTS.

In all the trenches, various sizes from fine rootlets to size of middle finger, various depths.

	Number of Specimens.
PEBBLE, USED.	
Battered on end, 6 in.	1
Total	1

PEBBLES, UNUSED.	
Depth, 9 in.	1
Largest, size of pigeon egg; smallest, size of pea (various depths)	75
2 in. below red film, under which chipped argillite was found lying flat, 16 in.	1
Large, pebble resting directly upon clay film, 17 in., 8½ in. long, 4¾ in. wide at widest, 2½ in. thick	1
Flat, 16 in.	1
Large, in Trench A, 15 in.	1
1½ in. in diameter, 4 in. under film	1
18 in.	1
Under film, 16 in.	1
Unused, large, in film, 16 in.	1
Small, 16 in.	2
Small, 16 in.	3
Most of the pebbles saved from 1 in. to 2 in. in diameter.	
Large, about 4 in. in diameter, broken, showing on one side, according to Mr. Kümmel, facets of wind erosion, in film, 14 in.	1
Weathered, 2¾ in. long, 2 in. wide, 1½ in. thick, resting below a distinct film and above another, 17 in.	1
2½ in. long on film, 16 in.	1
3 in. in diameter, Trench D, 6 in. on film	1
1 in. in diameter, on film, 18 in.	1
On film, 18 in.	1
Size of large chestnut on film, 20 in.	1
Smaller below film, 14 in.	1
Hazelnut size, 14 in.	1
Unused, 3½ in. long, 2½ in. wide, ¾ in. thick	1
Various depths	27
Various depths, Trench B	64
Various depths, Trench C	26
Lower part of stratum, various depths	16
2 in. long, 1½ in. wide, near three smaller pebbles	6

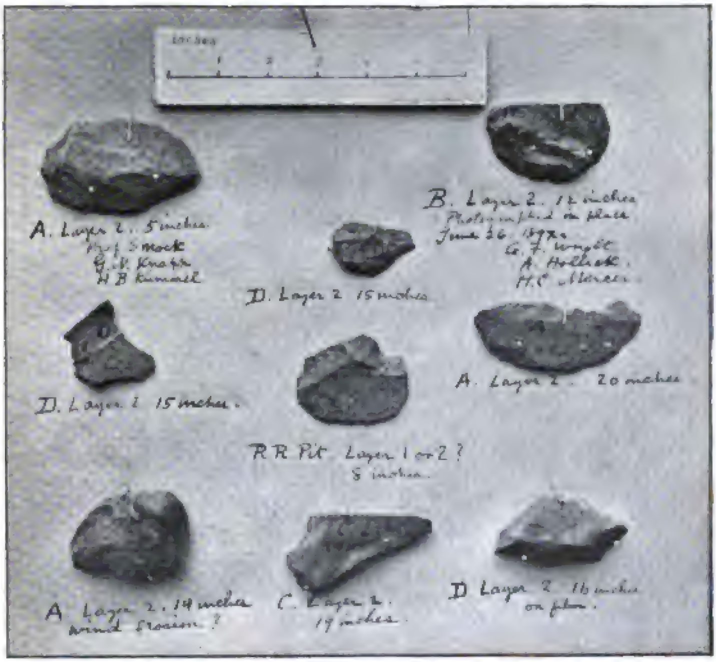


Fig. 3.

Fig. 3. Weathered pebble, artificial flake of chert and flakes of argillite, described in the text, except one on the upper left-hand corner, found at significant depths in the yellow sand, sometimes upon or under filmings of stratification, by Professor Wright and Dr. Hollick, Professor Smock, Messrs. H. B. Kümmel, G. N. Knapp and H. C. Mercer.

	Number of Specimens.
2 in. below red film, 16 in.	1
(Large, 7½ in. long, 5½ in. broad, 3 in. at thickest, broken, upper side roughened but not certainly wind-worn, C, 16 in.)	1
D, various depths	13
Total	254

PEBBLES, FRACTURED.

2 in.	1
4 in.	1
9 in.	1
Small fragment, Trench B, 9 in.	1
Small piece, probably broken by fire, 5 in.	1
In connection with black streak extending down from surface	2
Small piece, 2 in.	1
Total	8

MISCELLANEOUS STONES. PROBABLY NOT ARTIFICIAL.

Flat sandstone fragment, 18 in.	1
Jasper (?) fragment, 10 in.	1
Quartzite (?) fragment, 18 in.	1
Argillite (?), 13 in.	1
Unidentified piece, 6 in.	1
Flat stone, possibly chip, 12 in.	1
Argillite piece, vertical position, 6 in.	1
Argillite piece, 7 in.	1
Quartz piece, flat, 4 in.	1
Argillite piece, flat, 1½ in.	1
Sandstone fragment, 4 in.	1
Shale piece, 2½ in.	1
Quartz fragment, 2½ in.	1
Red shale fragment close under faint red clay film, 18 in.	4
Argillite chip, 12 in.	1
Shale fragment, 3 in. diameter, 18 in.	1
Argillite fragment, Trench C, 10 in.	1
Quartz, depth not marked	1
Quartzite fragment, 6 in.	1
Argillite fragment, 6½ in.	1
Total	23

ANT GALLERIES.

About size of string-bean pod, not so long, 1 in. below irregular filmings, penetrating film by narrow hole, size of timothy grass, D, 16 in.	1

	Number of Specimens.
Size of lima bean, under clay film, just above which is another similar gallery, containing eggs, Trench D, 22 in. .	1
½ in. horizontal diameter, ¼ in. vertical, 1½ in. below middle film, followed inward and lost without tracing upward through film, Trench B, 11 in.	1
Total	3

In the study of these interesting facts, the position of this comparatively clean stratified yellow sand, which we have called Layer 2, first and last demanded our attention. Resting immediately upon an unquestioned deposit of the Trenton glacial gravel, and immediately under the familiar culture layer of the Indian how were we to explain its construction and gauge its antiquity? How were we further to account for the existence in it of a series of objects, betokening human agency, challenging attention at various depths, and scattered without sign of collocation? Did the man-made objects in the sand and those in Layer 1 above the sand, pertain to the same epoch and refer to the same manufacturer? Were we dealing with two human culture layers or with only one? Was the sand, stratified though it appeared, after all of recent deposition? Or, if ancient, had the objects found in it intruded themselves into it from the superficial Layer 1 in modern times? These were the questions that exercised us as one by one we removed the specimens in Layer 2 from their resting place. After considering the various possibilities suggested in a summing up of the evidence above cited I reached the following

CONCLUSION.

Fifty stone flakes mostly man-made, not of argillite alone, as we had been led to expect, but of argillite, chert, jasper and quartz, one battered pebble and two fragments of anthracite coal, thus positively found in the much discussed stratum of yellow sand which we have called Layer 2, present us with interesting evidence as to the relation of the sand stratum to man; while in the further presence of twenty-three miscellaneous stones, and two hundred and fifty-four water rolled pebbles, ranging in magnitude from the size of a chestnut to that of an ostrich egg, we are confronted with interesting considerations as to the deposition of the sand, whether by wind or water.

The antiquity of man, at the site judged by these observations, depends on three questions:

1. *Are the significant chips in the yellow sand artificial?* To which my experience answers, yes.

2. *Are they in situ in the yellow sand?* To this I would say that the notion of the intrusion of objects from the Indian surface layer above (Layer 1), down and into the sand below (Layer 2), is suggested to me for the following reasons: Because the deepest artificial specimen in the yellow sand (Layer 2) rested not over three feet below the surface; because the

range of stone chips (argillite, jasper and quartz) and fractured pebbles was identical in the yellow sand (Layer 2), and the Indian layer (Layer 1), and showed thickest under the latter, growing thinner downward; because argillite chips identical in character and equally decomposed and patinated occurred in both layers; because two pieces of anthracite coal were found in the upper part of the sand (Layer 2), and, lastly, because the artifacts were scattered at irregular depths in the sand (Layer 2), nowhere suggesting by their collocation a floor of occupancy or workshop abandoned by primitive man.

On the other hand, neither the shallow depths of the objects nor the closeness of the layers exceeded conditions known to archæology where divergent culture epochs had been found to rest closely one upon another. And while no potsherd or bone was found below (in Layer 2), the similarity of the stones used where the blade material (ingredient to glacial gravel) had continually remained the same failed to overweigh the probability of antiquity as to the specimens from the lower layer. Coal fragments not uncommon in the alluvium of the Delaware, whose trough traverses coal beds near Mauch Chunk, had been found by me in an underplaced Indian village layer, probably pre-Columbian, at Upper Black's Eddy,[1] so that the presence of two small pieces in Layer 2 offered no demonstration of the modern intervention of white men in the construction of the layer. Furthermore, the observed position of roots and the study of small ant galleries failed reasonably to account for the site of the chips in Layer 2, whose position in several cases beneath well observed and unbroken films of stratification remained the most important fact in the evidence.

When all was considered I was forced to conclude that a significant number of artificial chips rested *in situ* in the sand, and hence were of an age antedating its deposition.

Here again, as at the underplaced layer at Lower Black's Eddy, we were confronted by two thin strata of human occupancy, resting one upon the other, but separated from each other by an interval of time as yet unmeasured.

Antedating the familiar Indian, preceding the birth of the known riparian forest as indicated by the superficial blackness of its plant-stained loam, the immediately underplaced yellow sand Layer 2, close beneath the surface as it lay, testified to the previous presence of a chipper of argillite, jasper and chert, and a bruiser of pebbles, upon the surface of the bluff. As before remarked, the bones of animals and pottery, characteristic of the upper layer, were absent in the lower; otherwise, no marked difference appeared in the kind of stones used, their patina, or mode of fracture.

From the evidence previously produced by the more notable explorations of the Trenton site, the present testimony differs in several particulars. The shallow zone of discovery, ceasing at about three feet below

[1] See researches upon the antiquity of man in the Delaware Valley and the eastern United States. Publication of the University of Pennsylvania, Vol. VI, p. 78. Ginn & Co., Boston. 1897. F. K. Swain, Doylestown, Pa.

the surface, failed to encroach upon the region (10, 20 and 30 feet deep in the immediately underplaced gravel) alleged as the horizon of previously excavated human relics. No series of ovate blades resembling the drift specimens of France and England appeared as in previous years, to outclassify all other objects; while the contention (unreasonably overvalued in our opinion) in favor of the absence of jasper in earlier human horizons was not sustained by our discoveries. Nevertheless, the existence of man upon the bluff top under topographical conditions differing from those of the present had to be admitted.

This granted, however, the question of human antiquity at the site depended not so much upon the evident priority of the sand relics to the Indian remains of the surface loam as upon the length of time of such priority—namely, upon the final question:

3. *What was the age of the sand?* Extending outside the province of my qualification, this question remains to be settled by my colleagues. Let their special experience decide whether this yellow layer resting exactly upon the Trenton gravels is a true part of the composition of the latter. Let them inform us whether this overlying sand was the work of the river swollen to excessive volume by the melting of the great glacier, whether modern rivulets, since drained away, spread it upon the terrace top, or whether during the time before trees grew and loam formed, wind whirled it, pebbles, clay films and all, upon the even table of the bluff?

The following paper by Professor Hollick, prepared for joint publication with my own, was not received in time to be read at the Detroit meeting, though it was intended for that purpose and is here given as an appendix to my paper. H. C. M.

APPENDIX.

By Prof. ARTHUR HOLLICK, Columbia University, New York.

ON June 25, by invitation of Professor G. Frederick Wright, the following party met at Trenton, N. J., to make excavations in the Trenton gravel terrace, and to examine any archæological material which might be brought to light: G. Frederick Wright, Oberlin, Ohio; H. C. Mercer, Doylestown, Pa; C. C. Abbott, Trenton, N. J.; Ernest Volk, Trenton, N. J.; Arthur Hollick, Staten Island, N. Y., and a workman to perform the rough digging.

Through the kindness of the Misses Lalor, a part of their farm, favorably located on the edge of the terrace, had been reserved for investigation. Upon the surface of this part an area about forty feet long in a north and south direction, by about four feet wide, was selected by our party for excavation. Our workman first removed the surface soil by spading and shoveling, to a depth of about six inches, and threw it to one side. This soil was found to consist of a fine yellow-brown sand, mixed

with humus and other carbonaceous matter, which gave it a prevailingly dark color. All objects thrown out with this soil were collected and listed under "A" and "B;" the first representing the preliminary spading, the second the shoveling.

The objects consisted of fire-cracked stones; unworked pebbles, from size of a cobble to that of a robin's egg; chips and flakes, mostly of chert, but a few of argillite; rejects; imperfect implements; a fragment of anthracite coal; a cinder; a piece of oyster (?) shell; a piece of modern pottery; unclassified stones, mostly broken or chipped. The entire collection apparently represented the ordinary refuse of an Indian camping ground, with a few indications of modern civilization.

At the southern end of the area from which the surface soil had been removed, a pit was excavated 4 ft. by 4 ft. by 3½ feet deep. The sides of this pit were carefully squared and showed the following section:

1. Disturbed surface soil, consisting of fine yellowish-brown sand and black carbonaceous matter, giving it a prevailingly dark color, 8–12 in.
2. Undisturbed fine yellowish sand, irregularly stratified, with streaks of red sandy clay carrying small pebbles, 2 ft. 6 in.
3. Floor of red sandy clay at bottom.

The line of demarcation between the disturbed surface soil and the undisturbed sand could not be sharply drawn. Streaks and small pockets of black soil often extended down irregularly into what was apparently undisturbed sand. A zone was therefore recognized, below the disturbed surface soil, which we agreed to call the "zone of doubt." All implements or objects found in this zone were listed under "C." The bottom of the "zone of doubt" was about 18 in. below the surface of the ground.

Below the bottom of the "zone of doubt" we agreed that the sand was undisturbed by human agency and that any objects found there must be regarded as having been deposited at the same time with the sand, or possibly intruded from above, in which case some evidence or indication of such intrusion should be apparent.

The following method of investigation was then pursued:

One person entered the pit and gradually cut away the face with a trowel. As soon as any object was struck the rest of the party were notified and the sand around it was carefully removed. The distance from the bottom of the "zone of doubt" to the object was measured and the object was then removed and examined. Each such object was immediately wrapped in a separate piece of paper, together with a memorandum of the facts in connection with it.

NOTES.

As far down as we dug we found roots of living trees, larva of June bugs (?), ants, and occasional disconnected spots or streaks of dark matter, which I took to be the remains of old decayed roots.

The undisturbed sand was found to be distinctly stratified and evidently a water deposit. Pebbles and gravel grains were not uncommon, espec-

ially in connection with the clay seams, and nearly all the chips and implements found were lying flat, although few were more or less on edge.

Nothing but rough chipped implements (palæoliths?) and fragments were found below the "zone of doubt" up to the time when I left (4 p. m., Saturday, June 26th), when about one-half of the main excavation had been made.

Supplementary pits were also started close to the main excavation in order that others of the party might be occupied in digging at the same time. The methods employed were identical in each case.

We failed to verify the contention that only argillite chips and implements are to be found in the undisturbed sand. Some jasper, chert and quartz flakes were also found, but argillite was the most abundant material represented.

One fine chert implement was photographed in place. The details in regard to this and other objects found in the undisturbed sand are described by other members of the party.

CONCLUSIONS.

The writer accepts the conclusions of competent authorities that the so-called palæoliths are of human manufacture and that the sand in which they occur is of glacial age. Changes of opinion in regard to the age of the deposits, or the conditions under which they were laid down, due to the fact that implements of human manufacture have been found in them, can hardly be accepted as legitimate argument.

After a careful examination there seems to be no doubt that this sand is a water deposit and that it had not been disturbed by human agency prior to the time when it was excavated by our party. There is apparently no break in the sequence of deposition from the coarse gravel below, through the fine sand containing clay seams, up to the surface soil, —the entire series representing successive periods of flood and sedimentation.

The only controversy which seems possible is over the question of intrusion from above and, in view of the facts now adduced, the burden of proof should in fairness rest with those who hold this view and positive evidence of intrusion be brought forward by them. Nor should it be considered sufficient that one or two cases of intrusion be proven. It would be extraordinary if some such should not be brought to light. What is needed is evidence in rebuttal in specific instances where it is claimed that palæoliths were found in undisturbed sand.

INVESTIGATION IN THE SAND-PITS OF THE LALOR FIELD, NEAR TRENTON, NEW JERSEY. By THOMAS WILSON, U. S. National Museum, Washington, D. C.

A MEETING was organized for the 25th and 26th of July, at which were present Messrs. Salisbury, Holmes, Cushing, Culin, and, on the latter day, myself, with Mr. Volk present both days, for the inspection of the sand pits in the Lalor field on the top of the terrace overlooking the Delaware river, just south of and adjoining Trenton. The interest centered in the question of the antiquity of man.

I have always been a believer in the possibility of human occupation in America at a much earlier date than was assigned to the North American Indian. Believing in this antiquity, I have been continually on the look-out for, and ready for the reception of, evidence which should prove it. I was gratified on the occasion referred to, to find evidence tending in that direction.

Arrived at the pits, we (Mr. Volk and myself) were soon joined by Messrs. Salisbury and Holmes. The pits had been opened in advance, some of them the day before, in the presence of the party, so that new exposures were prepared. The most cursory inspection showed three principal strata or deposits: (1) the black soil on the surface eight to twelve inches thick; (2) yellow sand about thirty inches in thickness, with irregular red streaks through it; (3) stratified sand and gravel for an indefinite depth, and believed to have been washed down from the glacial deposits of the Delaware valley.

The pits were opened through the two upper layers and holes were dug at the bottom temporarily for the purpose of identifying the line between the yellow sand and the gravel.

The principal, indeed the only geological question discussed on this occasion, was as to the origin of the layer of yellow sand (thirty inches). One party contended that the red streaks in it were evidences of stratification and the deposit was therefore laid down by water; the other party contended that the red streaks were concretionary, and that whatever stratification might have once existed was now broken up and lost. The resulting conclusion of those who did not accept stratification in the upper stratum was that, in the absence of stratification, no one knew the manner of its deposit, therefore one could not be certain either of its antiquity or that of the objects of human handiwork found therein. The conclusion of the opposing party was that the stratified yellow sand was practically the same age as the stratified gravels beneath it, and that the antiquity of the objects found therein was practically the same.

The pits examined were dug through the yellow sand and its face exposed from top to bottom. This face was smoothed and then pared down in thin slices, scraped, as it were, by the various individuals present, in search of objects of human industry. The stratified gravels below were not investigated, and further than the general recognition of their glacial

origin, they were not considered. No question was made by anybody during the entire *seance* as to paleolithic man, or his implements or anything which could have belonged to him; indeed, the word "paleolithic" was not spoken by anybody.

The investigation pointed to the following conclusions: First, that the entire examination had no bearing upon the Paleolithic Period, nor upon the existence of paleolithic man, nor on any of the objects of his industry. The stratum of glacial gravels, to which paleolithic objects are claimed to belong and wherein they have been found, was not examined nor considered. Second, while there may not have been any stratification in the yellow clay, and so it may lack that evidence of antiquity, yet it furnished objects of human industry almost universally of argillite; while the layer of soil on the top, and such as was within the reach of cultivation, yielded implements or objects of human industry, of jasper, flint and quartz, and rarely of argillite.

Whatever differences there may have been in the geologic conditions of the stratum of yellow sand from the others, it bore archæological evidences differing materially from the layer above it. Argillite objects were found throughout that yellow sand stratum from eight to ten inches beneath the surface, down to thirty or thirty-six inches, while they were rarely found in the black soil stratum above. Jasper, flint and quartz were found within the first foot beneath the surface, but were rarely found in the yellow sand.

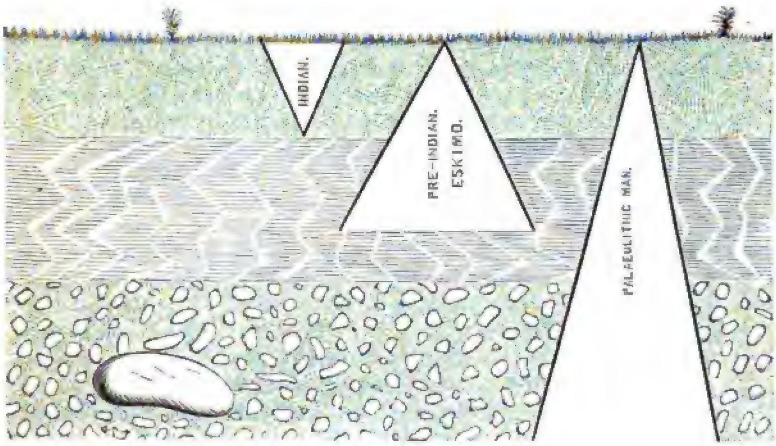
This difference between the products of the two strata furnished a line which the archæologist could follow with ease and certainty. The objects from the upper layer were practically the same as those found on the surface throughout the entire neighborhood, if not the entire State, and possibly United States. The objects found in the yellow sand, the stratum beneath this, were distinctly different, different if in nothing else than material. The differences were sufficiently marked to show different cultures or different conditions at the time of the deposition of the two strata.

The investigations made upon this occasion were entirely in harmony with, and vindicatory of, the position taken by Dr. Abbott in the Popular Science Monthly, January, 1883, Vol. XXII, p. 381. His statement then in regard to paleolithic man is to be omitted, as it was not considered on this occasion. One may doubt, if he pleases, his conclusion as to the existence of evidences of Eskimo industries, but confining his statements and his diagram to Indians and pre-Indians, no reason is seen why every visitor of last week might not have written what Dr. Abbott wrote in 1883, and might not have made the same diagram as shown in that paper.

Dr. Abbott, writing of the black soil, says (page 318):

> The flint implements known as Indian relics belong to this superficial or black soil, as Kalm terms it. Abundantly are they found near the surface; more sparingly the deeper we go; while below the base of this deposit of soil, at an average depth of about two feet, the *argillite* implements

occur in great abundance. The accompanying diagram more clearly sets forth the conclusions at which I have arrived, after years of careful study. By this it will be seen that, as the depth *increases*, the number of ordinary flint implements of Indian origin *decreases;* and that the reverse is true of the paleolithic implements which are a feature of the gravel beds; and is true of that intermediate form which is characteristic of the stratum of sand capping the gravels and blending insensibly with the surface soil. This intermediate form, which is always made of argillite, is both in workmanship and design a marked advance over the paleolithic implements, and yet is so uniform in pattern and so inferior in finish, when compared with the average flint implement of the Indian, that it has been assigned to an earlier date than the latter, and considered the handiwork rather of the descendants of paleolithic man.



Doctor Abbott's illustration of 1883. From Popular Science Monthly.

What is held to be convincing evidence of this has already been given in the statement of the relative positions of the two forms—Indian and pre-Indian—as seen in sections of undisturbed or virgin soil.

As before remarked, paleolithic man was not in question during the meeting in Lalor's field. When the evidence of the past shall be collated, and the evidence of the future gathered from the stratified glacial gravels which lie beneath the investigations of Monday, and paleolithic implements be found and shown, then the existence of paleolithic man in that locality will, doubtless, be admitted. At present, the examination of Monday went no farther than to show an earlier human occupation of the territory with a different culture, at least a different industry, from that of the modern Indian.

DISCUSSION.

THE CHAIR: So end the formal papers on this subject, and we now pass to the general discussion, in which all members of the two Sections are invited to participate. The Chair begs permission to call attention to the fact that no question whatsoever has been raised concerning the accuracy of the observations so admirably brought before the Section by Professor PUTNAM. There is no question as to the trustworthiness in every respect — as to the complete integrity of the archaeologic investigation. The only questions of importance before the Sections are questions relating to geologic interpretation. The Chair begs to suggest that the discussion be confined to the geologic aspect of the case. There is no person more competent to open the general discussion from the geologic standpoint than the Vice President of the Association, and Chairman of Section E, who shares the platform with the Chairman of Section H. I will ask Professor Claypole to open the discussion.

Professor CLAYPOLE: I feel that if I were to do this I should take up time that might be used to better advantage. My remarks might not be in the line of the discussion, and it might introduce irrelevant matter if I should bring in evidence from outside districts. I would ask the Section to allow me to give up the post to some one who has worked in the region. I would prefer to withhold my remarks until those who have been in New Jersey have had an opportunity to speak.

THE CHAIR: The subject is before the joint Sections for discussion.

A MEMBER: I have had correspondence with Dr. Abbott in reference to this location. I do not learn that any extinct animals have been found in this layer. If we had found any extinct animals, we might conclude that it was palæolithic. The point of pottery, that is an evidence upon the other side. It has not been found so far as I know.

A MEMBER: I would suggest that an invitation be tendered Professor Penck, of Vienna, who has made observations of a similar nature.

THE CHAIR invited Professor Penck to address the Section.

Professor PENCK then spoke briefly [giving an account of a place in Austria which had been buried by sand since historical time].

THE CHAIR: It is desirable that the discussion should not lag, and especially desirable that the geologists present should express themselves.

Professor WRIGHT: I beg leave to state a fact or two to correct some misapprehensions. First, in regard to what Professor Holmes says respecting the gravel near the Pennsylvania Railroad station in which Doctor Abbott reported finding numerous implements. Professor Holmes is certainly mistaken in representing that those gravels had been reworked by the waters of Assanpink Creek since their original deposition by glacial floods. Large numbers of boulders, some of them five or six feet in diameter, lie scattered all over the area as they were left by the workmen after having removed the finer gravel. I have numerous photographs of perpendicular sections of these gravel banks as they were exposed, from

time to time, during the excavations showing boulders two or three feet in diameter near the upper surface. The material has not been reworked. Professor Holmes' sections are not drawn from facts but from his own imagination. My photographs tell the truth. His drawings do not. If Doctor Abbott found implements in these gravels, as he said he did, they are as old as the glacial period.

As to Professor Holmes' examination of the sewer trenches, it is to be said that the conditions were not as favorable for the discovery of implements as he represents them to have been. The trenches were narrow and deep, poorly lighted, and were soon boarded up to keep them from caving in. It would be with the greatest difficulty that the implements could be detected along their perpendicular sides. Doctor Abbott probably saw twenty times as much of the perpendicular exposure of excavations in this gravel during the period when the Pennsylvania railroad was clearing away a space around the station as Professor Holmes and his assistants saw in the trenches.

With reference to the clayey stratum beneath which our implements were found in the excavations on the Lalor farm, I would say that, immediately before finding one of the implements described, I followed it continuously for ten feet working off all the material with the trowel before striking the implement. The stratum was throughout at least two inches thick. The analysis which I have given of this is not my own, but was made by an expert. Twenty-six per cent of this by bulk was clay, and only six per cent of that clay, *i. e.*, .015 of the whole, was iron. The amount of clay in that stratum was thirty-three per cent more than in the same quantity from the sand above. As to the specimen which Mr. Knapp examined, I should say that it was certainly not from this stratum, and so has no bearing upon this question. This is a real clayey stratum which had been undisturbed. It is impossible that implements could have worked through without showing signs of disturbance, and equally impossible that stones should have been brought up from beneath it by roots of trees without showing like signs of disturbance.

THE CHAIR: There are certainly several geologists present who are familiar with the Delaware Valley. If they do not speak spontaneously it will perhaps be necessary for the Chair to name them, but he would prefer not to be compelled to do so. Professor Chamberlin, the veteran student of glacial geology, is with us, and I know he has visited portions of this region.

Professor CHAMBERLIN: I have been urging for some years that those only should discuss these questions who had given very careful examination and thought to them, and that the thing which we need more than any other one thing is extremely careful observation on the ground, and extremely careful interpretation. Now, while I have been at Trenton, and have seen a little of these formations, and perhaps am prepared to appreciate what others have said, I am not prepared to make any contribution of my own, of any value. As to the interpretation I have indicated at various times certain general criteria of interpretation but these have

been all involved in the discussion to-day more fully than they have lain in my own mind. There has been really no contention in the discussion to-day, on the basis of any recent evidence, that anything pertaining to the unquestionable presence of man has been found in the unquestionable glacial deposits. We have some forty or fifty feet of unquestioned glacial deposits, amply exposed. That contention, as indicated by Professor Holmes, has passed out of the discussion. There is, therefore, this significant thing in the discussion to-day, that with an ample exposure of glacial deposits it is not seriously claimed that human relics are found in them. We may therefore infer from the discussion to-day that the battle on that ground is a matter of past history. The question, therefore, is narrowed down, as it seems to me, to uncertainties in three or four particulars; and this perhaps is the most wholesome state at which we could arrive as the result of long discussion preceding this meeting. I think all parties are convinced that there is a necessity for the very careful observation and the very careful interpretation toward which we are fast moving, and to which we have perhaps actually arrived. It seems to me in so far as this question is typical of the problem of glacial man, it should put all of us in an attitude of firm conviction that at present there is no positive evidence, and in that negative attitude we can rest. I think we are all agreed as to the general antiquity of man at some place on the globe. We of America do not question the presence of man contemporaneously with ice in Europe. We have come here very recently, other races have come recently also. It is an open question when man appeared upon this continent. Therefore I think we have reached a wholesome condition. We may allow new evidence secured from time to time to stand on its own merits, excluding a large part or all of the present supposed evidence of the existence of man in America in the glacial period. I know of no evidence to-day that is of scientific value bearing on that point. We have the negative discussion to-day, after the careful investigation of the case. We may rest judgment while we go on looking for positive evidence under the new method.

Professor Claypole: I think we must all go away recollecting one thing, and that is our discussion relates solely to the New Jersey district. I think we must all remember that. It is a great point to keep discussions clear and definite. The second point is I think we must be on our guard against the misuse of the word palæolithic. In Europe that has a clear, definite, sharp meaning. Third, in regard to this clay, I do not think any geologist would call its structure stratified. I think the red clay is oxidized. It may be seen in a great many dirt banks. The oxidation passes down and even into single stones. That process may be seen in hundreds of stones, in which the oxidation has gone in from the surface and is preceded by a black zone. I am sure Professor Salisbury is right in saying that the structure cannot be true geological stratification.

Professor Holmes: I wish to ask one question in regard to the formation of such bands. Suppose they should form in recent sand. Suppose that iron begins to accumulate. I imagine that the porosity would be

in a measure reduced; and that since all this sand contains clay from the surface down, as shown by Professor Wright, why would it not happen as soon as the band of iron begins to form that the clay carried down by water would lodge upon this hardened layer making it actually a layer of clay? And so with each band of iron, may it not accumulate clay in that way?

THE CHAIR: Will some of the geologists please reply to the question specifically?

Professor SALISBURY: I had intended to say a word more about those red bands. I think the answer to Professor Holmes' question might be affirmative, though I could not point to any individual illustration of the process. The concentration of iron oxide along a given zone would certainly tend to check descending clay. Professor Wright insists that these red streaks are zones of clay, though made up of three parts (74%) of sand to one (26%) of clay, counting the iron oxide as clay. This is not my idea of a clay layer. I was impressed by the irregularities of the red bands, and by the fact that many of them were like the irregularities on a dune surface on which there were tufts of grass. The irregular bands looked as though they might represent old surfaces, on which vegetation grew, and on which reddish dust had been blown. This seemed to me a possible explanation of these streaks, but I am not insistent that it is the real one.

Professor PUTNAM: Will you please explain once more where that brick was found?

Professor SALISBURY: It was sixteen inches from the surface.

Professor PUTNAM: Our line of doubt is just above the upper red layer.

THE CHAIR: The Chair can hardly refrain from asking whether the distinguished archaeologist who comes to our shore from Scotland, Doctor Munro, will not say a word in connection with the subject?

Doctor MUNRO: I have come here for information, and the special object I had in coming to Detroit was to hear this discussion; and I beg to say that I have never listened to a discussion conducted on such admirable methods as the present. Since I am on my feet, let me say one thing: Mr. Myres has referred to these corrugated bands — I would call them layers. They are very common in all sand dunes and other wind-blown deposits, and we look out for these beds of iron for the purpose of trying to find the lost arrowheads. In regard to the origin, of course some have said that this deposit is due to wind, and others that it is due to water formation. Both may be right; because, when sand is blown up there, occasionally floods come, and when these floods pour upon the matter the water makes a stratification there. The probability is that it was due to wind and water work combined. I think that while I have been condemned for stating that the American anthropologists and geologists were doubtful as to the existence of palæolithic man — I think my impression is correct, and I still remain skeptical in regard to palæolithic man in America.

THE CHAIR: The Chair is strongly disposed to ask for one more geologic opinion. We have with us one of the foremost among the

geologists, not only of this country but of the world, in the person of Mr. Gilbert, the Nestor of the Geological Survey of the United States.

MR. GILBERT: All that may be said on the geological side has already been said. With your permission I would like to say something on the archaeological side. Although I am a geologist by profession, once for half a day I was an archaeologist. It happened that in Colorado I was able to examine what had probably been a village site at some date uncertain. The locality is one where the underlying formations contain fragments of a peculiar kind. Among these are no materials available for the use of Indians in making implements. Now, the uppermost deposit on this was sand, and it happened that that sand had been cultivated and the wind had blown away all that was disturbed by the plough, and there lay all the pebbles that had been on this surface and which had constituted the materials used by the Indians; they included flakes, arrowpoints, the hammers that were used in striking flakes from hard stones, and implements of other kinds that had not been flaked but ground into shape; these were scattered over the surface and I gathered a peck of them perhaps; but in searching for these I found probably ten times as many stones which had no indication of having been used by man. The presumption is that those pebbles had been brought by human agency. I wish to suggest the possibility that in this case also, in the sands near Trenton, the pebbles may have been brought by the same hands that brought the implements.

THE CHAIR: Although the hour of adjournment has passed, it seems appropriate that each of the gentlemen who contributed formally to the reading of papers be given a moment for final discussion. Professor Putnam will be the first.

Professor PUTNAM: My present feeling can be expressed in a very few words: I feel well repaid for all the work I have done in this connection, during the past twenty years, by the interesting discussion that has been brought out this afternoon among the geologists and archaeologists.

I am going to agree with our friend Doctor Munro that there is a good deal of doubt about palæolithic man in America; that is, from the consideration of the question in the discussion this afternoon. We have had a statement that a trench was dug thirty feet through the gravel, and that not a sign of the work of man was found during the digging of the trench. Mr. Holmes says that the trench was carefully watched, for five weeks, and nothing was found. Two gentlemen, watching the same trench, state that they found two chipped stones in the gravel as it was thrown out.

If any geologist had found a stone implement back of this boulder (pointing to the diagram of the river bluff), would you not consider it in place in the gravel, or would that still be doubtful? It was in such a position that I found a stone implement. I removed the boulder and there found a chipped stone implement. That seemed to me to be good evidence of an implement in place.

For several years there was a vertical section of gravel where the Pennsylvania Railway Company was taking away the gravel. That

section, like the vertical section of trench C, was watched almost daily, by Doctor Abbott and others, and during that time seven or eight (I have forgotten the exact number) specimens of chipped stones were found at different points on that vertical wall, as the Company dug away the gravel.

For over twenty years this gravel at Trenton has been watched whenever an exposure has occurred. During these twenty years we have secured over thirty specimens, found at depths below one foot in the gravel; and also hundreds of specimens taken from the talus. It is on the evidence of these specimens that we consider it possible that man may have existed during the glacial period in America.

There is one very important specimen to which we have not alluded. It is the human skull discovered at a depth of thirteen feet in the gravel on the site of the gasometer at Trenton. This cranium shows that it was knocked about at the time of the deposition of the gravel; and it is of a peculiar type unlike the Indian skulls found on the Atlantic coast.

I will not detain the Section except to say one thing more. We began this discussion by considering the age of the sand deposits at Lalor Farm. It has been said that perhaps the objects found there had worked down from the surface by agency of burrowing creatures; it has been said that these deposits had been all broken up by the action of various animals, tree roots, etc. If this is the case, we have to consider how long it would take the ants and worms to bring the sand to the top. We have to consider the immense length of time it would require to turn over this whole deposit by these means. From all that has been said to-day, it seems to me that a considerable antiquity is admitted for this deposit. That is all the archaeologist asks for,— that the antiquity of these objects (90 per cent of which are made of argillite) be admitted, and that these argillite objects be considered as belonging to an earlier occupation than that represented by the objects of jasper and chert and pottery found in the upper deposit. I think it is a grand advance if we have made it clear that the argillite culture is old and that it preceded the recent Indian culture. We have thus returned to the statement made by Doctor Abbott, in 1883, which has been confirmed by the later work.

Professor WRIGHT: The discussion this afternoon has pertained, as Professor Putnam has said, to the recent investigations at Trenton, nothing being said about the previous evidence. I want to call attention to one point made by Professor Salisbury that the roots of trees had broken up all stratification; why didn't they break up the red band? The roots of trees have always been going down there. Whatever you may say about that band, that is one of the things to be explained. I followed it through for twenty feet, but certainly there has been no disturbance of the stratification since those implements were found below that red band. I am not sure as to what those facts may mean. We have endeavored to present the facts so that you may interpret their meaning. Argillite culture was during a period that preceded the other, and that the red band of stratification of which 26 per cent was clay, and a very much larger pro-

portion of clay in it than sand, accumulated along that line over the argillite flakes. I do not believe the pebbles could have come up in any of the natural ways spoken of. I would say that when Mr. Kümmel completed his work on the first day his impression I know was that it was a water deposit. We may vary in our conclusions, but I wish a committee might be appointed to make investigations and report a year from now to follow the points made to-day. I think we owe it to Professor Putnam and others. I wish a committee might be appointed for that purpose.

Professor HOLMES: I object to the introduction of the expression "argillite culture" which implies a well differentiated period and people. I think that in the end we shall have to attribute all the works of art found in that section to the Algonquian Indian.

Professor SALISBURY: In reference to Professor Putnam's question if we would not regard the implement found underneath the boulder as having been in place in the gravel, I would say, most emphatically no. The river undercuts the bluff. Most emphatically anything found behind a boulder on the slope would be open to great suspicion. In answer to the question why were not the red bands disturbed, I would say they were. If they are of later origin than the sand itself, as is possible, they would have been subject to proportionally less disturbance.

Professor CLAYPOLE: The evidence that we have been discussing this afternoon is, as you know, entirely in regard to Trenton, and on this evidence the case will stand or fall. I fear myself, unless some new evidence unknown turns up, it will fall. Many of the members of the Section are aware that I have brought evidence from another part of the country, which I do not say is absolutely conclusive. but which is free from many of the objections brought forward in New Jersey. A cliff is a doubtful point, but if in other parts of the country there are places where there are no bluffs, then I think we may get evidence which, if it proves it in that part of the country — I speak of Ohio — would render it probable. I confess I share the doubts of a great many whether the man we have traced is the palæolithic man of Europe.

THE CHAIR: The Chair, in congratulating the section on an exceedingly interesting discussion, would like to call attention to a single point, and that very briefly. It is this: It has been well brought out in everything that has been said that, as search has gone forward and as observers have multiplied in the Trenton region during the last fifteen years, so the supposed evidence of glacial man, or so the supposed conclusiveness of the evidence, has grown weaker so far as that locality is concerned. Fifteen years ago there was hardly an archaeologist who did not regard the Trenton region as affording conclusive evidence of glacial man; to-day the manner in which the evidence has been torn to shreds is apparent to everyone. What I say relates only to glacial man. It seems to the Chair that it is fair to all parties, and may be especially useful to those members of the Sections who are not experts in such matters, to give this summary of the discussion.

ABSTRACT FROM THE RECORDS OF THE SECTION.

THE Detroit meeting of Section H was opened August 9, 1897, the President, W J McGee of the Bureau of American Ethnology, in the chair. The Secretary-elect having resigned, Dr. Anita Newcomb McGee was elected Secretary.

The principal business of the meeting was transacted on August 10, and was as follows:

1. A report of the informal conference of the section held December 30, 1896, at New York, was read by Miss Alice C. Fletcher, retiring President. This report, prepared by the Secretary, is hereto appended.
2. The Section voted to request permission of the Council for the holding of a formal meeting during the Christmas holidays of 1897. The permission was promptly given by the Council and $25 was allowed for the expenses of the meeting. Ithaca, New York, was selected as the place of meeting, because the Society of Naturalists is to meet there.
3. After the reading of Dr. Washington Matthews' paper on the Science of Ceremony the chair, on motion, appointed the following committee to coöperate with the author in considering and acting on his proposal to give a name to the science of ceremony: Dr. Washington Matthews, Dr. Stephen D. Peet, Miss Alice C. Fletcher and Mr. Frank H. Cushing.
4. The Sectional Committee voted to recommend to the Council the continuance of the Committee on Ethnology of the White Race in the United States. This recommendation was adopted by the Council.

At the close of the meeting of the Section, at 5 P. M., August 13, Miss Fletcher offered a resolution of thanks to the President of the Section, in which she called attention to the arduous additional work that had fallen on him as Acting President of the entire Association, in spite of which labor his attention to all the details of sectional work, his unfailing courtesy and suggestive leadership in the discussions, had been noteworthy. Professor Morse, in seconding the resolution, spoke also of the impartial presiding and the harmonious character of the meeting, as largely contributing to make it one of the most successful in the history of the Association.

ANITA NEWCOMB MCGEE, M.D.,
Secretary of Section H.

REPORT OF THE INFORMAL CONFERENCE OF SECTION H, DECEMBER 30, 1896.

AT the instance of the special committee appointed to promote the interest of Section H of the A. A. A. S., by request of the sectional committee and with the approval of the President and Permanent Secretary, an informal conference of the members of the Section was held at Columbia University, New York, on Dec. 30, 1896, for the special purpose of discussing the future work and interests of the Section, and to

consider the expediency of recommending to the A. A. A. S. the holding of winter meetings of the Section.

Miss Fletcher, the President of the Section, presided and about twenty-five members of the Section were in attendance. The morning was mainly devoted to discussing the relation of anthropology to other sciences and to considering its scope.

The Section considered the advisability of obtaining the authority of the Association for holding winter meetings of the Section. The general opinion was that semi-annual meetings of the Section would be useful to anthropologists and would tend to strengthen the Association. The following resolution introduced by Dr. Boas was passed:

"The members of Section H assembled at an informal conference on Dec. 30, 1896, in New York, suggest to the Section the desirability of holding meetings of the Section in winter at the time and place when the Society of Naturalists and Psychologists meet, and request that the suggestion be acted upon at the first opportunity."

An informal report of the Committee on Ethnology of the White Race in the U. S. was presented and the following motion in regard to it was carried: "In the opinion of this informal conference a half-day session of Section H should be devoted to the report of the Committee on Ethnology of the White Race in the U. S. and to papers connected with the subject."

In the afternoon Dr. Brinton spoke on "The Relation of the Psychical to the Physical," Professor Ripley on "Influence of Environment on Color of Eyes and Hair." Dr. Boas followed with a paper on "Growth," in which he presented some practical questions which show the vital bearing of anthropometric research on the every-day mental and physical health of school children. Incidentally he mentioned the desirability of authors printing measurements of individuals in full instead of merely giving the results. This idea received the hearty approval of other workers in this line as by this means future investigators are able to test the results and methods employed.

A committee consisting of Dr. Brinton, Professor McGee and Dr. Ripley, was appointed to consider anthropological nomenclature.

During the meeting notice was received of the death of the honorable Horatio Hale, formerly chairman of the Section and one of its most productive workers. Resolutions of regret were framed, a copy of which was forwarded to Mrs. Hale.

HARLAN I. SMITH,
Secretary.

SECTION I.

SOCIAL AND ECONOMIC SCIENCE.

OFFICERS OF SECTION I.

Vice President and Chairman of the Section.

RICHARD T. COLBURN, Elizabeth, N. J.

Secretary.

ARCHIBALD BLUE, Toronto, Canada.

Councillor.

MARCUS BENJAMIN, Washington, D. C.

Sectional Committee.

RICHARD T. COLBURN, Vice President, 1897.
ARCHIBALD BLUE, Secretary, 1897.
W. R. LAZENBY, Vice President, 1896.
MARCUS BENJAMIN, Washington, D. C.
HENRY FARQUHAR, Washington, D. C.

Member of Nominating Committee.

WILLIAM H. HALE, Brooklyn, N. Y.

Committee to Nominate Officers of Section.

Vice President and Secretary; and LAURA O. TALBOT, HANNAH WINGATE; MARCUS BENJAMIN.

Press Secretary.

ARCHIBALD BLUE, Ontario, Canada.

ADDRESS

BY

RICHARD T. COLBURN,

VICE PRESIDENT, AND CHAIRMAN OF SECTION I.

IMPROVIDENT CIVILIZATION.

A plea for the application of scientific methods to the amelioration of socio-economic defects and disorders.

THE responsibility you have seen fit to place upon me, I will now ask you to share with me.

The controversy in respect to a bimetallic money standard, and the other as to the limits of safety for representative or currency money, are certain to be fully worked over, by the powerful vested interests concerned, in Reports of Commissions, and printed volumes. I devote a minute or two to explaining that they are but parts of a far greater question of Metrology; one also requiring, for its elucidation, a more exact knowledge of the laws of thought than we at present command.

When we speak of value, equivalency, wealth, risk, trust, distrust, panic, prosperity, we are dealing not with concrete substances like gold pieces, but with states of mind; yet these ideas lie at the foundation of commercial exchanges and monetary science. We can measure the relations of one commodity to another, in a rough way, by the difficulty or labor-cost of production; but when we try to measure the relations of one commodity which has little or no skill wrought in with its production with another in which there is inventive or artistic skill, or sentiment, or

risk of life or limb involved, the relation is not merely quantitative. To illustrate: Have any of you ever imagined what would happen if some modern Rosicrucian were to succeed in doing what so long baffled the alchemists, and which has been announced from time to time as being accomplished, viz.: the turning of base metals cheaply into gold? No one can maintain that this is impossible; and this is preëminently the era when the dreams of ancient philosophers become realities. The diamond, a much more unpromising object, has been made before our eyes by M. Moissan. Such a discovery would introduce into the world of commerce, and indeed into all fiscal relations of men, an appalling confusion: first, by a general rise of prices; and, second, by a dislocation of fixed payments of interest, salaries and otherwise. Among other curious results we should witness would be a change of sides, and tunes, between the advocates of the gold and silver standards with a general desire to shift over by the holders of contracts for specific payments "in coin or its equivalent." The same thing would happen, only more slowly, if a vast deposit of gold ore was unearthed; and if, after gold were thus discredited by a practically inexhaustible supply, the attempt were made to put silver in its place (the price of which would be enormously enhanced), this state of things would be liable, in its turn, to be upset by similar discoveries. I am not sure but the after-benefits to mankind, and especially to labor, by precipitating the necessity of inventing some more efficient tool of exchanges, a scientific and more stable enumerator of values, would compensate for all the disaster it would temporarily cost. Shall we have to wait for such an accident for the settlement of a monetary system?

In leaving aside these more or less transient studies, we do not escape from money questions. On the contrary, economics have become so interwoven with our whole civilized activity and speculation, that money has come to be accepted as a measure of these states of mind, as well as of quantitative relations of commodities. For example: in a general way the per-capita consumption of sugar readily indicates the desire of the population for sweetmeats (a psychic phenomenon) and their ability to gratify it (a material phenomenon). A decrease of the average Bank Clearing House exchanges is a merely quantitative statement, but its relation to the increase of suicides, and the decline in marriages, of which it is also a faithful index, is not so obvious. The difficulty with

monetary science is that values or prices are subject to the rise and fall of tides of their own, to droughts and floods as compared with each other, and as compared with the conventional standards (regardless whether the standard be single or double) ; but the standard itself is adrift, moving now landward and now seaward, according to the caprices of that unstable and surface current, public opinion, and also to powerful undercurrents by those monarchs of finance, the arbitrageurs, whose hands are on all the productive industries and for whose benefit the rest of mankind exists and labors in unconscious servitude. Monetary science is not lawless, but its datum-points are not yet so fixed as to admit of easy reference. Many other problems of science have been worked out of similar complexity, and our task is not quite so hopeless as the usage of centuries might suggest. Many things have become possible within the past seventy-five years which seemed impossible prior to that period.

The markets of the world are becoming, for practical purposes, one. This is noticeably true of the credit market, or as it is usually styled, the money market. The economic needs of the United States, as indeed of all the American peoples, as I see them, are not greater abundance of circulating promises to pay but more of the staple commodities in world-wide demand in which to redeem the debts already incurred. This is equivalent to saying that we should get out of debt, and have something left over of the nature of quick assets which we can part with to the rest of the world as occasion requires. It goes without saying that as to families and persons, so as to nations, our possessions must consist of something besides bric-a-brac and apparel, the fashion of which changes and the value is soon lost. The luxuries we buy from the European markets would bring but little if shipped back there ; in fact, without our demand, the prices would be lowered. Champagne, laces, fine woolens, feathers and silks are poor property to raise money on elsewhere. If we would get credit or money, or the valuable substance that stands behind money, we must owe less and have a greater store of the articles the world needs. Whether the present estimation of gold as the measure of exchange values is excessive or irrational, it is a fact to be reckoned with. It follows that the surplus should be concentrated commodities, portable, exportable, and not too fragile or perishable in their composition, not subject to caprice of fashion,

nor of restricted demand, and of these the precious metals and stones have, by universal consent, best filled the requirements.

Our present civilization is lopsided; its contour is asymmetrical; it is not abreast of the knowledge of the time, and is not yielding to mankind nearly the amount of comfort and well-being it might be made to do. From a great number of social ills, defects and shortcomings, due chiefly to this overlapping of the childhood of the world upon its adult stages, I select a few of the more serious, which will require many centuries to correct themselves, in order to raise the inquiry among you whether it is not within the compass of human endeavor to accelerate a better state not merely to gratify an altruistic impulse nor in fulfilment of ethical ideals, but as a deliberate choice of divergent policies.

I. THE WASTE OF WARFARE AND ARMAMENT.

Ahead of its logical order, I take up the waste of war and constant preparation for war, which has haunted mankind, with few and trifling exceptions, as a malign heritage as far back as we can trace. History, whether printed in books, written on parchment, engraved on monuments, or burned in clay tablets, seems to be mainly a record of combats and glorification of warriors. Truly enough, the overrunning and subjugation of one community by another of alien looks or speech was one of the most impressive and awful calamities, surpassing in its mental impress that of earthquakes, eclipse, drought, forest or prairie-fire, flood, or insect pests, because strong sinews and courage availed to give relief in one case, but not in the other. Training to arms was the ordinary occupation of life, and death by wounds, or from privations in camp, the exit, while the greatest honors were paid in primitive as in modern times to warriors.

We need not resort to any superstitious legend to account for this combative instinct; it has its analogue among the brutes which bristle up and stand on their guard at the coming of a strange figure and, in the time of scarcity, fighting for the available supply of food, shelter or females. It is even so with man; finding himself within a zone or clime-bound belt of fertility subject to periodical encroachments of the ice-cap from the one direction, and to the fierce suns and tropical growths on the other, hunting and fishing his principal pursuits, agriculture being nascent, there

were ages when subsistence was precarious, when along with the *fera naturæ* he was driven to the caves and trees, and had to contend for his very existence against organic and cosmic foes. The character of combatant, strengthened from generation to generation, became a first nature; he must be ever on the alert for a foe and for an occasional rival. Essentially gregarious, man could not, until very recently, associate in large numbers without engendering an artificial struggle for existence superposed on the natural.

Until the dawn of systematic agriculture, the food products of a given terrain could not keep pace with the population; not until the dawn of modern navigation was the drawing upon far distant or more fertile regions practicable; the extinction of the feeble and unskilled was therefore a foregone conclusion. Even so lately as the time of Malthus, it was held that, nature having by her scheme of fecundity provided twenty partakers to her table spread for ten, the excess must disappear in some way by struggle or disease. It was not surprising that both philosophers and statesmen could reach the conclusion that every generation, or each quarter of a century, must have its great war in order to thin out the population to the capacity of the soil for sustaining it, or, otherwise to conquer enough more territory for the purpose. The tendency of this war spirit, thus kept alive, was to diminish the population at one end of the series in order to add to it at the other. It is a late discovery that this guaranty of the sufficiency of subsistence can be more easily and effectually obtained without fighting than with it. Aside from mutual jealousy of neighbor nations, or the fear of subjugation by one or more, the policy of fighting, in order to live, can be shown to be a colossal blunder. Man needs no longer to exterminate his own species to escape starvation.

Had the ordeal of arms remained as it began with a biting, scratching and wrestling, not much above the wolves, in which brute strength, sharp claws and teeth, and endurance prevailed, the perpetuation of the more warlike, and the extinction of the peaceable, must have operated to keep man closer in physique and mind to the brutes. A series of inventions of weapons—the sharpened spear, then the bow or flint-tipped javelin, the arrow, the projectile, the fulminating powder, the domestication of beasts and

birds—all so many triumphs of mind in the control of natural forces—enabled men to ward off the cosmic dangers and to beat back every other possible foe. Hunger and thirst and cold he had to contend with, as they had; soon he had their skins to warm him; he need not hunt his dinner before eating it; he could entrap it, and later employ the tame animals to help.

This great emotional source of wars, mutual distrust, suspicion and aggrandizement, remains with nations, as with men. When each was his own advocate, judge and executioner, the fear of combat was less than the fear of declining battle—the dread of a charge of cowardice. May we not look forward to a time when war growing out of distrust or wounded vanity shall become as obsolete among civilized people as the hunting habit, the duel, or pugilism, to which it bears a close resemblance. Perhaps if we can demonstrate the absurdity, the folly and waste of it, we may do something to banish oppressive armaments; but the war-impulse is scarcely amenable to reason, or to considerations of profit and loss; it is more vulnerable to ridicule and to the banishing of deep-seated prejudices. This power of fear, rivalry and suspicion being an inherited mental trait, dating far back in man's career, nurtured by song and story, embellished by poetry and art, stimulated by a religious enthusiasm, will die slowly. Like the beliefs in fairies and witches, they are not to be uprooted by argument alone but must be outgrown.

The devolution of the fighting trait may be traced, where one would least expect to find it, among the females. Women, young and old, higher and lower, instinctively, as we say, admire physical bravery, often in preference to moral courage. The very same badges and insignia of war stir the fierce emotions more than in the males. The showy uniforms, music and bearing, the plaudits of victory, rouse them to unwonted enthusiasm. Favor is extended to the side of the conquerors, disfavor to the vanquished, as eagerly as compassion on the wounded and dead. Unconsciously it may be, women are great aids to the recruiting sergeant, and to the gladiatorial shows, pomps, pageants and circuses. The songs of all nations reflect this powerful stimulus toward battle. "J'aime que le militaire," "The bold soldier boy," is the tenor of them; and, as if to intensify and keep alive the belligerent instinct, the boy children are still given weapons as playthings. Very rare are the

plaintive songs in denunciation of war. I remember one such in fashion in the first half of the century. It ran something like this:

> "If I were King of France, or still better, Pope of Rome,
> I'd have no fighting men abroad, no weeping maids at home,
> But the world should be at peace; and if kings must show their
> might,
> Then should those who made the quarrels be the only ones to fight."

Traces of the same surviving habit, grown dim from disuse, are found in the fainting or swooning disposition at the sight of flowing blood. A variation of this may be found in an attack of frenzy, or fury, from the same cause. This trait is also noticeable in some animals which are excited by the red color alone. The expression "war fever" is apt; it begins in delirium and ends in lassitude and exhaustion. The persistence of the belief that "blood letting" is the natural cure for personal or social ills, is a survival of the same kind. The passion for military prestige—*là gloire*—which so long haunted the Romans, and later the French, and is now conspicuous with their neighbors, is a fatal inheritance which lingers along with great intellectual power.

When one speaks of "firing the national heart," appeal is made to this latent instinct, and it rouses the inbred emotions in much the same way as its opposite emotion, the stampede of panic. Something of the same cruel homicidal impulse is to be seen in the old-fashioned "hue and cry" against alleged infractions of decency, loyalty or sacrilege and in the craze of mobs for lynching. Strategy and artifice may avail to divert it, or lead it to harmless issues, but it cannot be extinguished at the onset by threats or any reason short of that of exhaustion by superior force. This accentuates the danger, at all times along frontier lines, of an incident of encroachment or insult which may serve as a spark to ignite combustibles. The great concern of statesmanship is to keep this latent tendency from flaming into open war. "The cancer of a long peace" no doubt reflected a state of society in which industry was dislocated, when possessions were held by the strong arm, which indeed must have been wretched to make a state of war more tolerable.

Man will fight to expel intruders upon his domain, to resist capture, to overcome sexual rivals, to protect his family and to preserve his altars and property. Modern wars spring more frequently

from the latter than from all other causes combined; to be secure in the enjoyment of property, excuses and renders possible the armament of advanced nations as a reserve of police force, to maintain domestic peace and order, although the supplies are generally voted after artificial war scares. Holders of wealth are willing to be taxed to ensure tranquillity at home, even at the risk of personal conscription, rather than be left at the mercy of mob violence. Such is the irony of our civilization that some of the leading nations manage to combine a treble profit by fomenting or permitting wars in which the combatants become customers for arms, ships and munitions, and also for loans of money.

An appeal to force, to establish and maintain any degree of equality or inequality of fortune, is a mistake. It puts force in the place of equity; fixes right by might; moreover, it fails of its object; armament for domestic purposes excites alarm among neighbors, who have similar pretexts, and thus the Sisyphean task is kept up. Besides, it tends to invest property-holding *per se* with a sacredness which ought not to belong to it.

The maxim of aggressive statesmen, that "constant preparedness for war is the best security of peace," is a seductive, dangerous half-truth; it is about equivalent to the old adage that every gentleman should spend an hour a day with foils and pistols to keep himself in practice against intruders, assassins or robbers. Some ultimate appeal to force, of course, there must be; but, like the enforcement of decrees of courts, it should rather be *in posse* than *in esse*.

The world presents, at the close of the century, three very instructive object lessons in the policies and prospects of three continents—Europe, Africa and America. When the first Napoleon made his menacing prophecy that "in fifty years Europe would be either all Cossack or all Republican," he indicated correctly enough the two opposing forces between which it is kept in unrest and arms. He simply underestimated the time for working out. The six great powers waiting for the recovery, or expiring gasp, of their feebler neighbors, in order to obtain a share of their estates, is a sad spectacle. How many more furious struggles is history to record for the control of the Levantine shores? What a satire on civilization—not to speak of Christianity—to find the youth of Europe armed for such wanton waste! Turkey, Austria, Greece, the Balkans, Spain, Holland,—is their fate to be absorption into

a stronger empire? and if so, into which, and for how long? The epithet "cockpit of Europe" is one of warning to other continents.

We are permitted to identify Germany with the dominating policy of Europe because it is setting the pace, assuming the rôle of fencing-master and armorer to sister states (and shamelessly supplies them with weapons, money and tutors to carry on the strife); and because it so successfully combines mediæval feudalism, dynastic government, and state-church with the most advanced science and knowledge in the arts. Hence we find chancellors and reigning emperors alike claiming the usual kingly commission from Deity, the favor of the Almighty, but at the same time making open treaties with one set of powers, secret treaties with another set and, as if in distrust of all such allies and auxiliaries, providing also the heaviest battalions and best artillery.

How unfortunate, in one sense, is the situation of the African continent in not being permitted to receive the blessings of European civilization without its attendant curses. How much better it would have been to have consolidated the whole into one, or at most two, great commonwealths with a single republican form of government, one standard of loyalty, one language, religious toleration, common jurisprudence, freedom of internal commerce, facile postal and personal intercourse, uniform measures and coinage; instead of the partition into so many reproductions of European differences of flags, creeds, politics, customs, usages, speech, each with its patch of shore-front and a vast tract of hinterland to be fortified, defended and jealously watched in perpetuity. It is as if by inoculation the pest-virus were poured into youthful arteries. The picturesque old cradle of Mediterranean civilization, now lying isolated from, and yet so close to, the barbaric types, deserved some nobler treatment from the enlightened powers than this selfish spoliation. The opportunity to dedicate deliberately one quarter of the globe to peace and culture apparently has passed forever.

The dream of a peaceful consolidation of nations — a political millennium — is quite old. Fortunately, there are other potent forces at work making for peace. Good will and good neighborship are also inherent emotions. Among these may be reckoned the comity of nations, perhaps also religious propaganda, foreign trade and intercommunication. The late Secretary Blaine set a notable example of the peace-making impulse when he addressed his

eloquent words to the convened representatives of American Republics, counselling unification of policies, arbitration of disputes between themselves, and urging again the Fathers' doctrine of "America for Americans."[1] That invitation, for obvious reasons, omitted the Dominion of Canada. In any future conference it is to be hoped Canadians will see their way clear to participate, in this, as in other prerogatives of self-government, as they have a joint interest in the welfare of North America politically, scientifically and economically.

That the international comity, and if you please the jubilee features of the year may not be neglected (in which, however, Science is thrust far in the background), as an American response to the very hospitable suggestion of Professor Dicey — probably drafted before the failure of the general arbitration treaty — of a common citizenship for the people of Great Britain, her colonies and offshoots, the United States being specially included, let us say: unworkable though it be, it is received as an expression of good will. I venture to offer instead the counter proposition that we might have with Canada a Zollverein treaty abolishing fortifications, fleets and custom houses along the four-thousand-mile frontier, letting the tariff revenues be collected at the seaports. Reciprocity of language, traditions, laws, coinage, metrical systems, postage and railroad conveniences we have. By a larger interchange of merchandise and ideas, together with the freest intercourse, the benefits to be derived by both parties are so great, that any sacrifice on either side would be insignificant, temporary and easily borne.

Modern warfare is becoming more and more a contest of ingenuity and material resources. Numbers or physique of combatants count for less, and even personal prowess is of less importance. Craft in strategical manœuvres remains as of old. Machines, explosives, transport, commissariat, surgical skill and adjustment of knapsack and accoutrements are now paramount. Success depends also on industrial production and intellectual power. National debts, formerly supposed to be aids to peace, have become incentives to war; financiers often find a joint interest with diplomats,

[1] This condensed motto is frequently misunderstood as implying a sort of hostile coalition against Europe calling for a hostile coalition by Europe. It is simply a forecasting desire not to be embroiled in European neighborhood disputes about boundaries, dynasties, creeds, alliances, easement-rights to ports, etc., from which America can keep aloof and should be encouraged to do.

purveyors and soldiers, in fomenting or permitting wars to go near the point of danger for dynasties and capital. How to escape from the meshes of this combination is one of the most portentous and baffling of problems of politico-economic science. Following the analogy of Guaranty and Insurance corporations, it might be well for Europeans to invite proposals from the House of Rothschilds and its affiliations for a stipulated sum for which they will guaranty peace between the several members in lieu of the present onerous exactions of war loans. The financing of governments has become so large a vested industry that it demands the right to live, like some other institutions we shall have occasion to discuss, and society can neither get along comfortably with them nor extinguish them without violence. Let there be started a rumor of an invasion, blockade, or a vote for new ships and arms, by one of the number, the cabinets of Europe are in instant trepidation. The legislative assemblies at once catch the craze — up go the budgets, out go the contracts for munitions, and war loans, and then fluctuation in the Bourses. Somebody presently discovers it was a fictitious or exaggerated alarm, and the agitation subsides, but the taxes remain just the same.

In Mulhall's recent work, "Industry and Wealth of Nations," are the figures of the national debts of Europe with an attempt to segregate that portion incurred for railroad and public works which latter are in many cases part of the military régime. The figures are impressive but fall far short of expressing the total money cost of armament, to say nothing of the loss of public and social morale. The burden of industry is thrown upon the children and aged women, increasing the hours of labor and lowering the scale of living.

With a due sense of humiliation, we must confess that no single remedy can be found for this unhappy tangle of affairs. Parliaments, by their Constitution, represent the opinions of ministerial governments, and are not themselves exempt from war-fright. It is a common trick of falling ministries to strengthen their hold on power by resort to menaces and scares. The presence of one unclouded, undaunted mind at the time of panic of the multitude is a priceless desideratum; it is as cheering as the advent of a cool, competent surgeon into a room full of hysterical bystanders around a prostrate patient. Presence of mind, ridicule, satire, caricature and especially comic cartoons are more likely antidotes than argu-

ment. The disease is in good part psychic and the remedy must be of the same kind.

Again, therefore, we may say science, including its socio-economic branches, is our best hope for peace and disarmament. The stoutest heart quails before a stream of electricity, hot steam, asphyxiating gas or explosives rained from an air-ship. Among the suggestions containing more or less of promise, the following are worth mentioning:

(*a*) The Swedish Professor Nobel, who accumulated an immense fortune from his nitro-glycerine inventions, has left a large fund to promote the extinction of war by making it so deadly that nations will be afraid to resort to it. Evidently when commanding generals are themselves brought within the range of unseen, and practically irresistible, dangers, they will not be so eager to seek this method of promotion.

(*b*) Soldier and civilian are alike interested in maintaining a high standard of health and efficiency, the former having more depending on his doing so. Selected on account of his physique, the soldier should have more than the average intelligence, and if the army regulations or the instruction of the superior officers teach him how to care for himself, to moderate his passions, and endure privations, it will be a small compensation for his detachment from the industrial ranks. Sanitary and personal hygiene are of the utmost importance to the perpetuity of armies, nations and races.

(*c*) Continuance of the diplomatic methods, cumbrous as they are, must be assumed, although their function is becoming dimmer in the presence of telegraphs, telephones, newspapers and popular participation in national councils. Diplomacy has been at all times under suspicion of insincerity and deceit; but it has the great merit of defining in language the grievances and causes of war which gives time for passions to cool. Armies are now massed while diplomacy is getting ready; collision comes before the causes can be stated.

(*d*) Courts of Conciliation are an improvement in diplomacy in that they also give time for passions to subside. They do not, however, prevent armament; and armaments are *ipso facto* provocative of hostilities. They will still be needed as against non-arbitrating parties. The binding force of treaties, not always scrupulously observed, is being weakened by the example of conspicuous breaches.

Great Britain and the United States may creditably vie with each other in setting the world an example of this mode of preventing and settling disputes.

(*e*). To the above, I would fain offer a further suggestion aimed at those who have the responsibility of deciding upon war or peace. The actual ruler, whether it be a parliamentary body, or a President or Chief Secretary, King or Chief Minister, Kaiser or Chancellor, Sultan or Vizier, should be required to obtain the sanction of some deliberative body, not of his own creation or selection. In addition, every such officer should be required, by fundamental law, to surrender his office, authority and emoluments into the hands of such an assembly, at intervals diminishing progressively with the duration of his reign; such council or parliament to be at full liberty to restore it, further limit it, or to confer it upon a successor. Such a check upon the inebriety of power and incident flattery is needed as a safeguard against aberration of intellect or perversion of moral balance. The temptation and strain upon the faculties of one who is the fountain of honors and promotions ought not to be imposed without some such restraint. The tendency to hero-worship is truly a psychological taint, pregnant with dangers enough in the populace and positively ruinous when reflected on a single mind not chosen for robustness.

In this momentous struggle of medieval types against popular forms of government in continental Europe, occupied by three millions of armed men, and seven to ten millions more subject to call, the Britons and Americans jointly owe a duty to the cause of civilization and peace, not to be swayed into the same mad folly.

II. DECADENCE OF RACES.

The abstraction of numbers by warfare and the privations of army life, vast as they were, do not account for the decline and degradation of the great empires of the past. Besides, decay seems to overtake the conquering as well as the conquered race; and there are instances where the enslaved have become in turn enslavers. Mere numbers are not strength nor tenacity of nations: witness the Hindoos. The old Greeks were not numerous, but what they lacked in numbers they made up in vigor and sagacity.

What are these other causes of premature decay? In other words, is there a natural term of life for races, as for individuals — a cycle of growth, maturity and senility? Dr. Charles Pearson, in his work

on the "Life and Character of Nations," has attempted to answer these questions and, in addition to other minor causes, traces the decline to an inherent difference of stamina or staying power in the ethnic divisions of mankind, corresponding to our present external classification of races by color of skin and hair. The startling conclusion he reaches is that the swarthy or dark-skinned races are destined to outlast, and of course supplant, the lighter-skinned or Aryan group. The evidence is scanty and inconclusive; but it is significant and carries us back directly to the interesting controversy now waging between Professor Weissman and his critics as to the quality of the germ-plasma and its transmission without impairment or improvement, into which we cannot here enter. The rival hypotheses of Galton and Cope deal, however, with problems belonging to the sociologist as well as to the zoölogist and microscopist.

The doom of the light-skinned races, according to Pearson, is fixed, for it is not to be averted even by admixture. The bearing of purity of race on its persistency is far from being worked out. So far it goes to show the active dominant types are mixed; while the purer races are few, isolated, and nearly stationary in civilization. Whether these differences be due to reversion of ancestral types—atavism—or to the greater complexity of organization and nerve-strain, is an interesting study which we must ask the biologist and somatologist to elucidate for us.

Just why the builders of Assyrian Phoenician Nile valley, Yucatan, Grecian temples, thousands of years ago, have not left their qualities to their descendants, may be due to other causes than the stability of the somatic cell or to intermarriage; for instance, denudation of forests, inroads of infectious or contagious disease, insect pests, errors in diet, the warrior occupation, or a combination of all in greater or less degree. The higher types of men seem to have arisen along the broken coast line, or in the moderately elevated regions. The great plains or steppes have not been favorable to density or quality of population or to courage or vitality. The liability to periodical prairie-grass or forest fires may have stunted the development of men and animals alike. Meteorological conditions are important to flora and fauna; the annual mean of sunshine, the precipitation of moisture, the range of extreme temperature, and the degree of humidity are factors of survival in the geography of races. The more northerly seem to prevail over those

nearer the equator,—provided we do not go too far north—due allowance being made for the modifying influence of ocean-currents and altitude above sea level, the lines being not exactly isothermal but hygrometric. Singularly enough there are notable exceptions to this rule. In the dry deserts of Arabia are to be found, among a population of Bedouins, chiefs of noble mien and splendid form, descended apparently without dilution from a remote ancestry. So in Abyssinia, almost a hermit nation, as Slatin Bey tells us, the natives combine great physical endurance and courage, under the most adverse surroundings of plant life and notwithstanding the general prevalence of syphilis, long supposed to be an escutcheon of civilized mixed races, and a very sword of destruction. The example of the Jews so frequently cited in favor of purity of race is important evidence, but complicated with other than ethnic factors, such as the Levitical code of hygiene, the rite of circumcision, the confraternity caused by ostracism, restricted occupations and social temptations, each of which plays a part in the endurance of races.

As a matter of fact, the population of those nations which make enumerations has largely increased since 1815; but this has been made possible by the opening of new sources of food supplies, for which exchanges of manufactures have been given. In spite of the migration of more than twenty millions to distant parts of the world (America chiefly), every considerable area has increased its own numbers; and it is only quite recently that France, the sole territory where emigration is practically *nil*, or less than the immigration, is found to be stationary or slightly declining. There is a well-founded suspicion that what is now happening to France will, in due time, happen to the others from the same causes. Is the fate of Rome, Carthage, Venice, Thebes, to be repeated? Macaulay's New Zealander sketching the ruins of London Bridge is prophecy alluring to the historian, but it also finds ready acceptance among social philosophy essayists, who offer, however, the most divergent array[1] of moving causes, such as the decline of marriage, vaccination, flesh eating, narcotics, condiments, degeneracy, iced drinks, sewerage, irreligion, destruction of caste distinctions, etc., etc.

[1] Mr. G. A. Read, for instance, finds in the proneness to alcohol and narcotics, an artificial ordeal exterminating those least able to carry their load of poison.

It was reserved for Mr. Brooks Adams to discover that the scarcity of the circulating medium (in this case the depreciation of white metal is in mind) was decimating the whole race.

Unless we assume that there is in each new birth a redemptive power, a dropping of the taints of parentage, the human race ought logically to have come to an end long ago. In some way, as yet obscure to us, in which natural selection plays its part, health must be catching as well as disease; otherwise the major and minor pestilences would have brought a quietus. Phthisis is a comparatively modern disorder. Cancer is another of the internal lesions coming to be known as induced diseases of the blood which may be carried about to all climes and propagated with fatal facility. Whether the special bacillus starts the decay of the lung tissues or follows as a sequence of the decay, seems to be still in doubt. Pathologists are just now enamored of the theory of antitoxine inoculation—a sort of tame medical ferret sent in to combat the invading rodent organism. If this is our best hope, the ravages of tuberculous, cancerous and febrile ailments leave civilized races but a short respite.

The researches of Sternberg and Metchnikoff into the function of the leucocytes as guardians of the normal state of arterial currents are more intelligible and logical,—if the hypothesis of phagocytosis shall be established—as this leaves the work of eradication of the blood deterioration in the hands of the organism itself, not wholly beyond human control, makes it largely an affair of metabolism of food into living cells with a counterpart activity of the emunctories. The function of the ductless glands in the formation of these blood cells is the corner of histology and pathology now awaiting special researches, and from which we may expect refreshing relief from the antiquated fetish theory of antagonizing drugs.

History concurs with physiology and with statistics in the view that civilization is not favorable to marriage and fecundity, though it may be more propitious for the rearing of offspring. In spite of the surcharge of sex-passion which nature has thrust upon men, and the equally enticing wiles and coquetry of women, most of whom must look to marriage as a career, it is more and more of a failure. Polyandry and polygamy are being crowded out by monogamy, but the philosopher is tempted to ask whether monogamic union and the family as we know them are also to disappear; and, if so, what will take their place. Shall it be a return to celibate asceticism, or a resort to the state as foster mother?

For some occult reason it is not as easy to be born into the world, now and here, under civilized conditions, as formerly under

semi-civilized. The proportion of still-born may be less in our days than formerly; it probably is, but the infant mortality is greater. The mortality of parturition increases alarmingly notwithstanding aseptic devices. One may well ask why the parturition of the *homo sapiens* is attended with so much hazard? Whether the civilized man habitually mates at a later period in life than the savage, after the pelvic bones and ligaments have lost much of their elasticity, or that a larger cerebral development of the modern infant, out of proportion to the bony or muscular framework, renders him less viable; or whether the replacement of the earlier *sage femme* by an accoucheur with his case of instruments and anæsthetics, is more responsible for the increased mortality, we leave to gynecologists to decide. The change of both sexes to indoor employment in shops and factories, rather than arduous labors out doors, accounts for some of the loss. The net result in grandchildren may be the same as for prehistoric man by reason of better care during the period of adolescence.

If our census statistics are trustworthy, prudential as well as physiological causes are at work in the same direction. Families do not arrive so early, nor in such quantity, as in primitive life. The perpetuity of the race is left to the unthinking classes. The aversion to child-bearing crops out (especially in large cities) in various ways. The practice of abortion, very common in Asiatic countries, and suspected to be very prevalent in Europe and America, proceeds of course from prudential or economic considerations, fashion, or avoidance of social penalties. We know that the advent of girl children to the Chinese and some other peoples is looked upon as misfortune; as might be expected, fœticide and infanticide are common. Fear of want, love of pleasures and varieties, dread of pain and risk of death, the handicap in matter of house renting, awe of the religious authority,—all play their part in this great matter of diminishing population, which has engaged the attention of the French savans and legislators, and the sacerdotal government of Quebec.[1]

The optimist queries: "Why worry about the extinction of the human race?" which reminds one of the American Plato's reply when told by an Adventist that the end of the world was at

[1] The birth rate of the New England states ranges it seems between 18 and 22.5 per mille while that of the far West states is still less, or less than that of France, and being lower than the death rate means, unless redressed, ultimate extinction.

hand, viz.: that "he could get along very well without it." The question whether perpetuity of race is desirable, is equivalent to asking whether anything human is worth preserving. It is answered by science and ethics alike: whatever may be the rights of the individual over his own life, the plain inference from study of nature is that parents exist more for the sake of children than children for parents; life is essentially a sacrifice of the passing for the coming link. The more serious query for us is to know how it may be lengthened, or extinction avoided. Not one of us cares to be of a declining or prematurely dying race.[1]

Gratification of the gustatory nerves, located at the back of the tongue (which is not at all identical with the appeasing of hunger), together with the convivial propensity of man, a corollary of his gregariousness, is responsible for a part of his shortened longevity. It has its double aspect of physiological and psychical influence. No people suspect their daily food, or beverage, to be harmful; for the most part they would as soon tolerate criticism of their religion, their patriotism, their wives, as their bill of fare, but each in turn freely expresses his contempt for the table of the others. Is there not some underlying vice in the habitual food of the civilized world, which, of course, includes its preparation? Of the

[1] On this very point an interesting and instructive bit of testimony has recently come to hand.

The opportunity of studying the aboriginal life of these interesting islanders in the Pacific Ocean is passing before our anthropologists, and physiologists have extracted the whole lesson for the benefit of learning. The British Government recently appointed a commission to examine into the decline of the population of the Feejee Islands. The proceedings, intensely amusing, might have been more instructive had it been composed in part of trained feminine obstetricians. The inquiry disclosed a birth rate surprisingly high, much higher than the average of Europe, and a death rate still higher, and all sorts of reasons were offered to account for it; as it was a concomitant of the coming of the Caucasian race, the onus fell on traders and missionaries. The testimony of an elderly accoucheuse, familiar with both conditions, revealed the fact that the native women had become indifferent to the obliteration of their own people, and this because the joyousness and *sans souci* had been taken out of their lives; a sense of sin had been introduced and these Gardens of the Hesperides had been turned into vales of tears and disciplinary plantations, workshops and hospitals for which the hopes of a celestial reward are deemed no compensation. The same lesson is to be drawn from the Hawaiian group; the advent of the superior race is fatal not merely to the life of the inferior race, but the sad grind of money getting, the worry of competition, and modern fashions is fatal to contentment.

Is it not even so, in a degree, with our competitive civilization and religious creeds in Christendom itself, and in Buddhist lands? Have they not cast an artificial gloom over lives that would be full enough of sadness without them? What a pity that some effort has not been made to discover the source of that uncomplaining stolidity of the red-skin papoose as I have often seen it, carried on the back of its mother; or of the general glee and absence of painful cries in the Japanese babies as compared with those of the Western lands.

grass seeds which furnish the staple for the bulk of the world, rice constitutes at least half. Christendom prefers the wheat, rye or maize, which we esteem as the superior grain; yet the Chinese and Japanese contrive somehow to nourish stout sinews, though more diminutive bones, and acute brains and courageous hearts out of the blander grain without recourse to much animal food. Their superior recuperative power in hospital against injuries and lesions of disease is notable. The conquering quality of British tribes is believed to be due to the ample ration of beef; of the German to his of beer and sausage; of the Mediterranean littoral, to their free use of wine. To modern science we are indebted for the explanation that the decoction of the coffee berry by the Levantines and of the tea leaf by the Mongols for ages has probably contributed to their survival, by supplying a boiled or fermented liquid, which was doubtless comparatively more free from morbific bacterial organisms than were the polluted wells from which the water was drawn, in those densely crowded and ancient abodes. The medical view that life is shortened more by over-eating than by starvation, in its ordinary sense, is confirmed by the chemist's laboratory tests, and by the spectacle of contrasted races. Can we not, while imparting our science, philosophy and literature to the Chinese and Japanese, take a lesson or two from them on diet, and perhaps on clothing and house furnishing also? Their comparative exemption from phthisis, insanity and neurasthenia alone should put us upon our inquiry.

A leading American physician has said, more or less jocosely, that the coming man compared with the present, will be a big headed, small bodied, puny limbed, bald, toothless, spectacled and toeless creature subsisting on concentrated foods, to which we may add the qualifying remark that he will not keep coming for any long period. The fate of that people where teeth and eyes decay, and dentistry and opticians flourish is not at all conjectural. It concerns the student of physiology and sociology alike to ascertain what causes are at work impairing the digestive organs, the teeth and eyes of civilized peoples, and in what respects the as yet uncivilized have a manifest advantage.

Making due allowance for the power of accommodation of the system whereby blood and tissues are made out of so wide a range of food-stuffs, the conviction forces itself upon us, seeing the effect of alimentation upon plants and animals, that while the norm

may be a shifting one from youth to age, there is a norm, and that deviations from it must tell upon the vigor and endurance of the race. The very general use of salted and smoked meats for example: has not that had much to do with the increase of gouty and rheumatic affections, usually attributed to acid fruits or wines? This practice could easily be dispensed with, by the use of cold storage and desiccation.

Then again, the civilized man, and especially the woman half of him, habitually lives in a warmer, closer atmosphere than the savage. Have not our air-tight houses, with their stoves, steam-pipes, furnaces and weather strips contributed not a little to diseases of the respiratory organs? Nay, the presence of a cellar under the dwelling rooms is a suspicious coadjutor. Then there are the germ-dust gathering carpet, curtains, portières, plush furniture,—are they not in some degree responsible for the spread of pathogenic bacteria upon tissues already weakened by defective nutrition?

An English physician, lecturing to a recent graduating class, as reported in the Lancet, ironically said, by way of caution against excess of confidence, that "the average life of a fact in physiology is about four years." Intended as a reproach upon the practising medico for his running after new therapeutic discoveries, it is also an encouragement and a compliment, that one error can be run down, and superseded, it may be, by another, in so short a space as four years. Alas! it takes a much longer period, on the average, to exterminate some of the so-called facts of social and political science.

III. PERNICIOUS COMPETITION.

Professor Cairnes happily hit off one of the most salient features of the literature of Political Economy, of the passing generation at least, when he styled it "a more or less handsome apology for the present state of things." One cannot but feel that it has been for the most part a thrashing over of old straw, and even now with enormous output of printed matter, there is very little beyond a rehash of the old controversies about wages, funds, rent, balance of trade, incidence of taxation and so on. Writers divide themselves into three classes, each of them defective without the other two: the historians and academicians who have gone over the writings of their predecessors and who know but little of

the business of the world; the statisticians who put faith in their ability to reckon up into tabular form, that which is incomputable, as well as that which is, and whose industry is out of all proportion to its value for scientific use; then there are the enthusiasts, who are profoundly dissatisfied with the present distribution of wealth or the conditions of industry, and who can propound schemes of reform and then turn the older arguments and figures round as upon a swivel in support of the ideals. Doctrinaires, who for the most part write the treatises and books about finance and trade, know but little of the world about them; while the bankers, arbitrageurs and movers of the world's crops and tonnage are too busy to write about what they understand. The result is the literature of political economy is about one generation behind the practice. Especially is this true of international finance.

The new world having a virgin territory to occupy and improve has run in debt to the old somewhat recklessly. The burden of interest and repayment of the capital is irksome. Extension of the debt at low rates is dependent on capacity to pay if demanded. So long as we are handicapped with this mountain of debt, it is in the power of a few foreign holders of our promises, or titles to property, to bring on a panicky feeling at any time they choose—though fortunately it is not for their interest to do so—but the time may come when it will be. Besides this hampering of debt, there has been an unconscious extravagance, of which we shall have more to say when we speak of the tendency of luxury. In a wholesale way, we have been exchanging our liens on and evidences of ownership of lands, timber, railroads, manufactories, mines, breweries and the like for shiploads of merchandise, the bulk of which we should have been the better for not having at all, and nearly all of which we might have made for ourselves. To get square will cost us hard work and self denial. With the exception of this broad distinction of debtor and creditor nations, and of the latter supplying articles in which there is limited competition, all the world is engaged in a general scramble in which cheapness of production is the goal. The industry of nations has developed into a species of hostile contest, not quite so hazardous as actual warfare, but demoralizing and exhausting in a less degree only. It is as if all marches were required to be forced marches; and all business must be conducted on the brink of the precipice of bankruptcy. It was always so within the confines of

a given territory; but the substitution of steam for animal power, and of dextrous machinery for handicraft, has not only intensified the competition within the old boundaries, but has also set the maritime nations to trying to undersell each other. In this contest for cheapness, standards of living, hours of labor, habits of frugality, depth of purse, vigor of body and acuteness of mind, all have their part to play, and the efficiency and economy of government, stability of institutions, probity of character, are also pitted against each other. Individually and collectively, an ordeal, growing more and more severe, confronts all trade, agriculture and manufactures. When the five hundred millions of Eastern Asia shall have grasped our mechanical inventions, it is idle to suppose the occidental standard of living can be maintained if the régime of unrestrained competition is to continue.

This is the crux of the labor problem, and also in large part of the commercial and financial problem. To the older writers aiming their arguments against the arbitrary and often absurd restraints of trade by statute, it seemed as if perfect freedom of trade was an ideal state of things, to usher in a millennium. The question has become too broad for local statutes. Freedom of trade breeds extravagance, improvidence, overproduction, followed by panic, depression, enforced idleness, discontent, and so on in recurring cycles. The struggle is too keen; it is the cosmic struggle for existence intensified, socially wasteful and destructive. Too many men are in trade; too many trying to "live on their wits," with the result that the peasantry of all nations are being worked too hard, and robbed of their share of the gross product. Less than half the population of Christendom is at work, half of those in agriculture, fifteen per cent in professions and public service, while ten per cent are in trade and transportation. Left to itself, we may be sure that by natural selection, the weaker in trade will be forced to the wall, and the few fittest will survive, but the competition will not stop; that must go on unless some general corrective is discovered. The tendency in domestic trade may be found in the mammoth Department Stores which are surely crowding out their smaller competitors, and this results in such dislocation of trade from its usual channels as to engage the attention of legislatures and turn elections. It is obviously the fact that in nearly every city or town there are about five times as many merchants in a given line of business as are necessary,

each with its corps of clerks, bookkeepers, delivery wagons, etc. If the saving of a concentration is shared with the customers, as it certainly is in practice, the community of buyers will have no reason to complain, but it is otherwise with the producers and manufacturers who must submit to the prices dictated by the big concerns, who are frequently able to take the whole product or refuse any. The power of concentrated capital and marshalling of labor, which has been going on in mechanical occupations, has now spread into trade, shipping, finance and even into educational pursuits. The smaller rivals are seeking shelter with the larger ones, by absorption or alliance to escape a worse fate, as we noted in the case of political aggregations. This has led to consolidations and leagues under the name of Trusts or other fiduciary contrivances. Railroads cannot escape from being dragged into this pernicious rivalry; they are under control of their patrons rather than of their stockholders. The legislatures, sustained by the highest courts, say they shall not combine "in restraint of trade;" though they are reluctant to cut each other's throats in competition, they are compelled to keep in the game at the same rapid pace as their customers. Regimentation and coercion in labor, hardly less intolerable than regimentation and obedience in militarism, are threatened unless some remedy can be devised. *Laissez faire* points towards a cruel and despotic struggle which is discouraging in the extreme; nor is there in the province of legislation apparently any adequate relief.

The step from a status of slavery to that of serfdom with a claim on the land for subsistence was important; the evolution from that to voluntary contract was equally so, and no people holding to either of the former systems of labor can hope to contend against the latter. But the régime of free contract is hardly a finality. The rankest injustice is perpetrated under its forms, all over the world, where proprietary rights are acknowledged. It is, of course, far better to require the consent of laborer and employer, but with this consent great wrongs are possible — are indeed common. This is a very grave question in social science — "how to put a curb on astuteness," as a magazine writer has styled it; how to shield the weaklings and credulous from spoliation by the crafty and unscrupulous. How shall the ignorant and confiding part of the population be safeguarded against overreach-

ing, temptations, wiles and wares set for them, and for each other, without opening the door to still greater evils? These lures are of all sorts and degrees from the knave who offers to sell counterfeit money, or lotion to beautify the complexion, to the banks, insurance and trust companies of many kinds; nor are the learned professions above setting traps for the unwary, or of suborning the press into becoming accessories. Look at the enormous outlays for advertising proprietary medicines, which debauch and befog the public conscience, if they do not injure the public health.[1]

It is not easy to suggest a rectification of the evils of unbridled competition, especially where statutory restraint is either aggravative or impotent. With some hesitation I venture to point out some of the underlying causes of our trouble.

[1] H. J. Davenport, in his recently published "Outlines of Economic Theory," tersely sums up the evils of competition in trade:

"The stimulus of private interest works out in a vast amount of crime and disorder which necessitates, in policemen, courts, juries, sheriffs and lawyers, the expenditure of social energies. Likewise in purely private affairs the expense of preventive methods against ill-faith and dishonesty is a weighty matter. Outlays of this sort would be relatively small in the collectivist system. There are large wastes of energy in competitive attempts to give to cheapness the outside gloss of value. Shoddy in cloth, paper in-soles in shoes, clay in soap, marl in sugar, not only waste the energy of putting them in, but largely destroy the usefulness of the honest product. Socially speaking all this cheapness is excessively dear.

There is a similar compound of waste in the enormous outlay for newspaper puffing and lying. The entire system, also, of marketing through agents and commercial travellers has in it large elements of waste. The excessive multiplication of middlemen generally falls under the same head.

The present system is also responsible for hordes of human beings living by their wits or their worthlessness — social make-nothings, paupers, vagabonds, speculators of useless types, prostitutes. Parallel with these are the respectable do-nothings, the leisure rich, the inheritors of wealth, the coupon-cutters. Within this class of respectable make-nothings must be reckoned also the valets and waiting-maids, the outriders, hostlers, servants and flunkies whose energies never work out in any utility, for which the world has any real need. And in a background of misery stand the unemployed, with whom, as misery, we are not at present concerned, but only as waste. Never an inconsiderable class, they swell in times of depression to an enormous army."

"Fashion demoralizes industry and fosters starvation. Warehouses are filled with commodities to supply a demand that has vanished, or to forestall a demand which never appears. Disaster and ruin result. A novelty strikes the popular fancy; there follow immense profits, intense production, multiplied factories, prosperous allied industries, growing cities, inflocking laborers, investment, speculation. Fashion grows cold when the commodity becomes cheap and plenty; then failure, closed factories, cancelled capital, collapsed boom, idleness, hunger and riot. Almost all industrial centres know something of this experience. All over the world there are Nottinghams regretting a banished lace industry. The foe of industrial peace is ebb and flow, change and uncertainty. Fashion in commodities is parent to business gambling, great fortunes, great losses, feverish activity, feverish lassitude, fluctuation and bankruptcy" (pp. 305 *et seq.*).

First, let me ask "why do men engage in and remain in trade; or enter upon any of the professions?" The usual answer is, "to earn a livelihood;" but this is not all. In civilization certainly there is the further motive of a desire to succeed, to become a master in the craft if possible, and whether or no, to amass some surplus of possessions as a provision for old age, to win society prizes, to dower daughters, start sons in business, perhaps found a house, endow a charity, build a monument, or leave a fortune, all of which motives are of the mind. We each of us see, or think we see, the wisdom or necessity for any one or more of these aims and purposes in life. Men and women are the slaves of ideas, and words stamp ideas so firmly on the average mind that they are with difficulty dislodged. What different ideas are conveyed by the words "success in life!" Not one in a thousand analyzes the thought to see its relativity or instability. If there were no need for a provision for old age, or for charity bequests, the extra industry would be needless too. If there were not the ambition to win, to excel, to outrun or outgather, the tremendous exertion of the rivalry would be stupid. Partial attempts are made to do away with the necessity for individual provision for the infirmities of old age by providing retiring pensions for the army, and in some countries for civil officers also. For those not directly in the service of the state, poorhouses (which are too often poor homes) are established by law; and some states now propose to aid by labor pensions also. These arrangements are the just and necessary sequel of a state of things where human life and vigor are sacrificed wholesale in the strife for cheap production.[1]

Second.—The eradication of the vanity of emulation, the desire for distinction, may be more difficult, but something might be effected in that direction. Suppose, for the occasion, that there were no prizes to run for, there would be less racing; I mean not material trophies, but the distinctions and adulations of winning in any contest, athletic, professional or social. If, by some happy

[1] Mr. Plimsoll convinced his government that near 2,000 sailors' lives were recklessly lost yearly through overloading. We may note in passing that the need of such provision is seldom found among proprietors of the soil, small or large; the farm is the savings bank and, in the hands of a practical farmer, a very good one. Neither is the working farmer often spurred by the ambition or vanity "to cut a figure" or win some distinction in the society ranks. If himself or family takes a premium at the county fair, it is as a sort of contribution to the common enjoyment, and not as a distinction of social caste, nor does he require the relief of pauper acts in old age.

contrivance, as much pains were taken to encourage confraternity and equality of estimation as are now taken to encourage leadership, and dissimilarity of estimation, would not much of the social strife and worry disappear? Within the family such inequalities are generally frowned upon. Can we not extend the ethics of the family beyond its pale to the whole social organism? The idolizing tendency of human nature is one not to be proud of or stimulated, but rather to be repressed. Why would it not be well to commence with infancy, in school or business, and abolish all prizes, honors and bribes of every sort for simple good conduct, or for doing one's best? Where there are winners there must also be losers; and for the latter there is very little regard outside the family, which discouragement is of itself conducive to further failure, bitterness, malice or suicide. The office should seek the man rather than go to the persistent intriguer or shameless "hustler." Aside from the fostering of an unwholesome sense of superiority, does not the whole practice of merit marks and competitive examinations of our schools and colleges work badly, in favor of a certain superficial readiness of mind which will have advantage enough over a less precocious maturity without conventional badges? The honors and emoluments of public and private station go together, a duplication of pay. It is one of the most sombre traits of the older civilizations surviving in great vigor, the readiness to tail into line obediently behind the nominal leader. When physical prowess was the surest road to distinction, the sway of one-man power was a necessity. In these days of deliberative assemblies of quasi-equals, it is a vestige of former subservience. Perfect equality of mind, or stature, we shall not have; but there is no justification for putting the tall man on stilts and lopping off the shorter ones. This is just what our present civilization is blamable for doing. The winner if only by a nose length, or by a scratch, is elevated out of all proportion to his excellence. If we must "play pretend" at all, why not minimize the differences between coadjutors, rather than exaggerate them? Some may declare that all men not being alike they cannot be treated alike. Certainly not, but we ought not to magnify or multiply the rewards for the superiority which, if it does exist, nature is sufficiently discriminative without the help of social man.

Third. Corollary to the provision for maintenance in old age

or disability is the necessity for some system of more constant and steady employment prior to decrepitude. At the best of times fully ten per cent of able-bodied laborers, mechanical or factory hands, are living in enforced idleness, and in times of depression, the percentage is very much higher. The difficulty here too is in part psychical. It is not enough that the laborer wanting work and the employer wanting work done succeed in finding each other; the latter must be satisfied not only as to the wage he can pay out, but also as to the character of the proposing laborer; he does not want to introduce discontent or disorder among his men. This trust or confidence is almost impossible in an idle population drifting across a continent. Without going so far as to affirm the right of the willing laborer, even if he have a family depending on his exertions, to employment by the state; still the community has him and his family to support in some way; why not do so in a systematic and economical way? It may be said that it would discourage saving habits, but the present plan does worse; it begets a lack of sympathy. If every county in each State (it would operate badly to have it in some and not in others adjacent) were to lay out in advance some useful public work, sufficient to employ the quota of discharged laborers, at a bare living wage, to prevent tumult and pillage, credit being resorted to if necessary, the positive benefits might not be so great, but the unseen damage might be averted. Central Park in New York City owes its existence to an impromptu politic stratagem of this sort; and there is not a city or county that might not resort to similar works of transport, embellishment or sanitary aids, greatly to its advantage, and thus mitigate the severity of panic waves.

IV. SPENDTHRIFT LUXURY.

Travelers in the Swedish-Norwegian peninsula concur in opinion as to the high general average of thrift and contentment existing in spite of the uncongenial climate, the paucity of manufactures for export, and the presence of an aristocracy and a considerable standing army. The extremes of riches and poverty are not so great. What a pity that this Scandinavian secret of contentment can not be made available in our more fortunate lands! True, there is a large consumption of ardent spirits, but the humid climate may account for it, or diminish its worst effects. Then the

ratio of illegitimate births is high, but this is not to be charged to moral laxity of the women so much as to restraint laid upon the marriage of the young men by Army and Church. No very complete explanation of this utopian satisfaction is offered beyond a pervading high literary standard due to excellent public schools; to a general and steady industry on the part of the whole population, male and female, young and old; and the absence of pretentious and ostentatious displays of wealth. The latter may be due to an anxiety to escape the tax gatherer; but it is refreshing to find one instance where the more liberal education coexists with industry and contentment, which it is fashionable in some countries to regard as incompatible. The same thing is true of Holland, which, however, lays the world under tribute by its floral tubers and Spice Island colonies; but Dutch thrift has made her a solvent creditor nation, and in time of stress she has but to sell her investments to draw from others. Other examples might be cited, Switzerland for one. The lesson to be deduced is the same: that industry, thrift, frugality, are the cure for hard times, for international competition, and contribute to good morals and stability of social order and progress.

By way of contrast take the example of Americans. There are, in Greater New York, at least ten thousand families besides the young patrons of restaurants and hotels to a much larger number, who will eat spring chicken and spring lamb in spring months, strawberries from March until June, and then take up some other premature fruit (without once tasting a really perfect berry), who are totally unconscious that they are indulging in any extravagance. The same class in cities all over the land demands the steak cut from the middle of the ox's anatomy and scorns that from before and behind it at half the price. The expenditure of the Americans for spirituous liquors is about the same as the British, and equals the cost, to the consumer, of the whole wheat consumption. The same is true of tobacco which is a masculine waste. The outlay for domestic service is a large item, and a considerable amount of it must be set down either to luxury or enfeebled homemakers. The loss from household waste is notoriously exorbitant. I do not grudge the household and farm help all the compensation it gets — for, if well and conscientiously done, the cook is certainly as worthy of his hire as the doctor — especially if the one would not play into the hands of the

other. It is truthfully said that the average French family would live from the waste and refuse of an American, and we might add that two Japanese families could subsist upon it. Nor is the lavish outlay confined to the table alone; it embraces nearly every item of expense. The aggregate cost of dress, in cities, may not be too large for the individual purse, but it exhausts what might be surplus for investment. So of house furnishing, pin-money, amusements and adornments. America, it is stated, takes diamonds equal to the whole product of the South African mines; furthermore the product could be greatly increased but is purposely restricted to what the world's markets will take at an upset price. In other words, the price of this commodity, we will call it, is "rigged" for us, while the grain, meats, and fibres we sell in exchange are parted with at prices made by competition with the poorest-paid labor of the world. Supposing a great war to break out and we should be obliged to realize on these "investments," as silly people pretend they are, they would not bring fifty per cent of their first cost to import. Nationally speaking, I suppose the gold and silver mining shares sold abroad will be regarded as a fair offset for any trickery of that sort.

Living, or rather working, in the commercial metropolis of the continent, where is landed eighty per cent. in value of the importations of fabricated wares, I am continually impressed with the prodigality of our expenditures for things which have little, or only a transient, value. The loss of money, great as it is, is only the smaller part of the injury. It is at least debatable whether it would not be true economy, after having paid for the goods, to throw them all overboard in mid-Atlantic—not at all on behalf of encouragement of home manufacture of similar articles, but to be rid of the pestiferous example and influence. These foreign articles not only keep the nation poor, but also debauch the public taste and conscience, set up false standards of what we can afford, introduce a succession of absurd fashions, and convert what should be stalwart men and women into mere fops and imitators. New York makes its living—a large share of it—in handling these importations, and distributing them widespread over the continent; and the greater its trade of this sort the more the country is ruined. Its press of course, from interest, if not from conviction, advocates the greatest freedom of trade, especially of so-called "objects

of art" whereon the profits are so large as to leave a wide margin for advertising purposes.[1]

Consider for one moment what we get: a collection of millinery, bric-a-brac and finery which sooner or later finds its way to the attic or the rubbish-heap; then reflect what we part with: the most precious part of our inheritance and labor, the phosphatic and nitrate constituents of the soil which do not quickly reproduce themselves like the ice taken from a lake, but are rather like the marble taken from the quarry. Deforestation may be followed at long intervals by reforestation, perhaps after damage by droughts and floods; the draughts on the fisheries (but not it would seem of the seal fisheries) may be made good by stocking, or by a close season; not so the soil, which must be replenished, or it soon ceases to repay the labor of tillage. Subsoil plowing cannot go on forever and adds to cost. The same cereals and breadstuffs we sell at bare eost of production, the railroads are compelled to transport at a rate equal only to the train-expenses, leaving the burden of interest on cost of construction to be borne by local or domestic traffic. The freight on a barrel of flour from points 1,000 to 1,500 miles inland to Liverpool is but a trifle more than to cart the same across any single city, and much less than to convert it into bread. Indeed, dealers have exhibited in Chicago loaves, made of Minneapolis flour, in Glasgow, of the same weight and cost as those produced at the lake port. It only remains to complete the *reductio ad absurdum* to reimport the Scotch bread in competition with the home-made article. This, however, is what we actually did for years with our cotton and wool fibres, sending them to Europe to be spun into thread and cloth, and would have continued to do so to this day if the arguments of the *a priori* economists could have

[1] The assumption that in order to sell its own products a nation must purchase a like amount from its customers is disproved every year by Brazil which effects triangular exchanges; and by the United States which liquidates its indebtedness by book credits, loans, and disposals of shares and titles to property. Western Europe has been swapping its fancy articles and superfluities to a tremendous aggregate for evidences of our indebtedness, calling for annual interest and dividend payments and tourists spending money of over four hundred millions. Whenever Congress is disposed to curtail this waste and bring the income and outgo into equilibrium, a chorus of importers and foreign ministers goes up that we are in danger of tariff retaliation. Having brought their easy-going customer to the verge of bankruptcy, they demand as of right a continuance, and resent any attempt on his part to get out and keep out of debt, as an affront to themselves. Retaliation may be left to correct itself.

prevailed. This is national improvidence; and its logical outcome is national dependence and national impoverishment.

By a provision of the Constitution, inserted at the instance of the more agricultural provinces, an export tax was forbidden to be levied. There has not been as yet much to regret in consequence; but one result is that any advantage which the United States may possess either in climate, soil, or beneath its soil, must be shared with other nations at the cost of extracting and transport. Our Canadian cousins are not so hampered, and are putting an export duty on logs and wood-pulp. Russia and America have the great workable petroleum fields, without which the cost of illuminants would have been enhanced. How long these deposits will last is a matter of conjecture, and they are obviously more or less of a speculative investment. While free competition was possible, there was a general rush to empty them upon the glutted market at whatever the oil would bring. The State of Pennsylvania would have been justified in expropriating the entire territory and conserving the product for the benefit of another as well as the present generation. It might do so yet with advantage. The alternative was a combination of small owners into one great excusable monopoly. When this precious deposit is exhausted, we shall have the slender satisfaction of knowing we helped to light and warm the world at no profit to the owners, unless the upbuilding of one or two large estates is to be accounted a compensation. A like history is to follow our depleting reservoirs of natural gas. The same reasons apply *pro tanto* to the anthracite deposits in a very limited area: parts of five counties in the same State which, with the present ratios of increase are liable to exhaustion in from 150 to 200 years. Anthracite coal will then be appreciated as a luxury.

We shall be told that it transcends the province of government to dictate what people shall like or dislike, or prescribe what they shall eat, drink, wear, or invest in; that the less it interferes with individual liberty the better for society, and so on. Partisan as I am of the largest individualism consistent with the equal freedom of others, and the welfare of society, I am loth to appear as the spokesman of social invasion of personal preferences. Our duty to truth obliges us to face the facts; and there are apparently two sides to this question. The protest of Mr. Spencer and his school against such restraints is, however, a claim that one kind of want is as rational and legitimate as another, and is equally entitled to

gratification. It is tantamount to saying one man has the natural right to tempt or inveigle another into buying anything he can palm off on him and, if carried to full length, would not except minors, imbeciles or inebriates. We know, in fact, that the rule is not universal. The difficulty arises in running the boundaries between restraint and freedom; but daily life and administration of justice consist in drawing such distinctions. The gambler, the lottery dealer, and the pawn broker desire nothing better than freedom; yet society does interfere with them. The underlying question for students is as to what tastes, cravings, instincts and aims contribute to the good of the race; which of them deserve to be encouraged, and which discouraged, by the State. On what other ground do all civilized nations tax spirits, tobacco and gaming implements?

In this matter of national, or race prodigality, take a familiar illustration: the country boy who has been saving up his scanty hoard for a whole year to visit the county fair, or it may be a seaside resort has no well-defined idea as to what his purchases will be for the occasion, no list of demands to be supplied; but it is certain as fate that he will part with his cash for what new or striking thing he sees, it may be gingerbread, a coasting-ride, or for street fakirs' wares. It is just so with the whole national merchandising. At the bottom of every country merchant's list, there is, in invisible ink, the further item: any novelty or trinket in our line that we can sell. A demand such as this is created and supplied by the same act, and articles are being urged and foisted upon callow customers far beyond their natural desires, beyond their proper ability to buy, quite in the manner of heirs to large estates, dupes of crafty cheats.

These views have been forced upon the attention of economists rather than propagated by them. Homilies against riches as hindrances to post-mortem rewards are plentiful enough; but they do little to curb ostentatious or competitive displays. It is not, however, against the truly rich the charge of profligacy is aimed so much as against those who would be thought so; by the masses who entertain the notion that respectability is an affair of outside badges, lavish outlays, and make-believe. Prodigality among the rich is responsible for much of this demoralization and ape-like imitation. Country homes and city mansions are tolerable, even though for the most part their owners are not living in them but occupying the

hotels of both continents. Yachts, racing stables, dog kennels, wine cellars, picture galleries, opera boxes, equipages, banquets, balls, costumes, jewels, are not the delights they are supposed to be by outsiders, but paraphernalia, the implements of a course of tiresome and exhausting exercises which society imposes on its votaries for no well-defined purpose, unless it be to excite the envy of those who cannot afford them, or, an elaborate system of matchmaking and place-hunting.

"Shall a man not do as he likes with his own?" is asked. That depends. Who does not know in the ranks of his acquaintance a proportion who, in good times and bad, by a generous table, or in showy hospitality, in dress or furnishings, devour the entire income week by week; people who might be comfortable but who never will be rich, nor yet acquire any competence for old age. The example of their neighbors and the multifarious temptations of the merchants are more than they can stand. Would you then destroy the inducements for acquiring wealth? Well, yes; some of them, at least. As already explained, I would encourage self denial, and thrift on the part of rich, moderate and poor alike, until we as a people get out of debt to others; and after that to establish good roads, comfortable, sanitary homes and pleasure-grounds; but not to enter upon a career of vanity and silly ostentation. I am glad to perceive in recent writers on economics a much clearer and firmer note than formerly on vicious luxury.[1]

[1] "The rich have their responsibility here and their duty. Wealth and culture have a special service in their saving influence toward higher standards of thought and life and away from the raging materialism of modern society. And note that the distinction is world-wide between luxury and ostentation. That which the rich desire for itself and not as the badge of precedence or the target for silly envy, they may well have — but only on condition that they rather hide than flaunt it. Society is greatly in need of lessons in plain living and high thinking. It is a fallacy to suppose the wastes of the rich are necessary for the employment of the poor. The consumption of the rich determines whether the laborer shall produce this or that, and not whether he shall produce at all. If the rich refrain in some measure from consumption, their savings profit society under the form of capital in the production of a larger social dividend. But the changing demands of vanity stand to society for more than waste and overwork. They corrupt art; they confuse and disturb the organization of industry. First, they corrupt art; no beautiful fashion, if once attained, is safe to stay. If grace and simplicity come as fashions, they go as fashions. The greed of novelty leaves the beautiful behind as antiquated, to be succeeded by the ugliness of humps and wings. From champagne to plumes of slaughtered birds, from skunk skins to jewelry, there is nothing permanent but novelty, no custom but change. And note that as soon as nothing in Art which is good can abide, there will be nothing really good. When the best work can have but a butterfly life there will be no best work."

"It is at first thought odd that unrest should especially mark the nineteenth century. The world is rich and growing richer, and wise and growing wiser. Never

V. THE BLIGHT OF PARASITISM.

Under Lycurgus, the pioneer social reformer, the Spartans held property in common. Satisfactory as the experiment was, it cannot be repeated, not even by imperial decree, without a supporting public opinion. To abolish individual property may not be possible now even by revolution, nor does it appear to be necessary or desirable, but some sort of restriction upon acquisition and transmission is desirable. "Can any man," ask the Socialists, "honestly accumulate so much as one million of dollars during his adult lifetime?" A slight acquaintance with either arithmetic or the investment of money at compound interest, or in the profits of trade, or agriculture, or pasturage, will show that he can. The familiar example of the blacksmith, who was to receive for shoeing the horse a penny for the first nail, two for the second, and so on, doubling for the whole series of 32, with a progressive doubling — not a remarkable increase for farmer, trader or stockraiser in each year — proves that he may become the owner of many millions, provided the rate of increase could be kept up for, say, a fifty-year period. The fructifying power of seeds and plants, or of domestic animals, forms the basis of about all the wealth of men and nations, that from mines, quarries and fisheries being inconsiderable. To a man not in either pursuit, it is practicable, as I personally knew a teacher to do, to lay aside one dollar per day between twenty and seventy, investing in good mortgages (formerly yielding seven and six per cent.) and

before would a day's labor bring so many dollars or buy so much. . . . . A larger, wealthier life is open to us, and it ought to be a greatly happier life. And yet we ask ourselves what does it all profit? Pass rapidly over in thought the question whether with all our centuries of achievement we are so greatly better off than the Greeks without. Is the greater rush and push of life a good thing in itself? What does it mean that the insane asylums yearly build larger for minds unstrung by tension? How about the multiplication of suicides? Likewise our prison populations are not disappearing as the good things of life become cheaper, but theft somehow grows out of plenty.

There is a grim paradox in civilization somewhere. Wherein do we fail, or waste, or misuse? How is it with all our opportunities? Our harvests somehow do not altogether shield us from hunger or our looms from tatters. What does it mean that as science grows and wealth multiplies the cry of poverty swells louder and louder, and that discontent is the fixed malady of our civilization? . . . . Splendor, no matter how much labor it has cost, is not splendor when it has become general. All may as well stand still as run in an equal race. Thus, material progress, so far as it is directed to competitive show, cancels itself in a strife for precedence. There is no share of gain in it for any one, which does not stand for discontent and heartache for some one else. All ostentation is waste from the point of view of society as a whole. For the poor it aggravates their poverty."— DAVENPORT: "Outlines Economic Theory."

the total is surprising to one who has not made the calculation, every dollar of it lawfully and honestly earned. The clamor against millionaires as such, which disgraces much of our social-science literature, to say the least, is out of place. The more of them the better—unless it can be shown that they have engrossed what belonged to others; or their use and disposition in some way contravenes the general welfare. It ought to be added that, the rates of interest now being lower, five and four instead of seven and six per cent. and the profits from merchandising, farming, grazing and many forms of manufactures and transportation being halved and quartered by the competition of the larger surviving concerns, the chances of becoming a half millionaire by mere saving, without resort to speculation or usurious rates of interest, are much more remote.

Suppose we take instead, the case of a person, it may be a bright capable young man, a young woman, or even an imbecile ward under guardianship, who inherits so moderate a sum as $10,000 during infancy, who, if he is fortunate enough to have guardians of judgment and probity, such a one may unknowingly become a millionaire without labor or economy. If one were able to add to this store further savings, and escape severe losses, the fortune might become very large, without encroaching on the rights of borrowers or lenders.

It is an interesting inquiry raised of late by the movement in favor of taxing inheritances now sweeping from one State to another; whether such an inheritance is honestly obtained, or if so, whether it is for the best interest of society that it so passes by descent or gift, to direct or collateral heirs. The answer to this class of questions is not to be derived deductively from moral or legal maxims, but from observation in practice. The reasons for a change in the policy of inheritance and descent of large estates I have already presented to the Section, at some length, and may be briefly summed up as follows:

First.—Under the régime of free competition, the desire to accumulate great wealth is not only laudable, but necessary. Business and investments are by it turned into precarious lotteries. An excess of care and fortune becomes necessary to guarantee maintenance during decrepitude, the rearing of surviving children, and support of relict, all of which might be accomplished with less capital if more complete assurance could be had. This I am persuaded ought to be and can be furnished by the State.

Second.—The further desire to secure by testamentary disposition

for offspring more than an even chance in life by setting the sons up in business, or of leaving portions or dowries to the daughters after death is neither so necessary nor so laudable, if we judge by the results. It cuts the nerves of self-reliance and engenders a feeling of "great expectations," of rewards that have not been earned; it encourages parasitism and creates sharpers and their dupes, stimulates fortune-hunters, and multiplies marital misery. Besides, it is contrary to good morals and sound policy to hold out the hope of advantage through the death of another, whether he be of near or remote kin. The profligacy of heirs to large fortunes, the demoralization and degeneracy of spendthrifts and their retinue need no amplification. As a contrivance to secure the marriage of daughters, it is remarkable at present chiefly for the success of the money bait in attracting the needy or degenerate holders of titles to nobility, and is no improvement on the plan mentioned by Herodotus where the more comely maidens were annually bid for at auction, the premiums going to the less attractive to whatever extent was necessary to secure a consort.

Third.—The difficulties attending succession are by no means so trifling as they may appear to those who are not put to the trouble. Public sentiment, following custom long established, dictates that a man's possessions shall be divided among his own children and blood relatives, with a reservation in favor of his widow, if there be one; and the statutes have been framed on that policy in cases of intestacy. The courts are more and more inclined to interpret ambiguous phraseology to that end, and to annul wills, or portions of wills, to the contrary tenor, except upon the clearest declaration of intention and choice of instrumentalities. This is an attempt to curtail that plea of "undue influence" which forms the basis of most testamentary contests, and which breeds scandals, family quarrels, and wasteful litigation. In olden times there was hardly a possibility of a large estate being passed without a liberal slice to the Church, and many States have expressly limited the share which may thus be awarded to those not of kin or to eleemosynary institutions; others have suggestively forbidden bequests to priest, doctor or counsel in attendance during the last illness. The drafting of probated wills reveals a disposition to withhold the principal sum from children, for which trustees are appointed, giving merely the income, the capital going to the grandchildren as being perhaps less likely to misuse it than their parents.

Fourth.—The bequests to charity, though undoubtedly in many

instances perverted to founding and maintaining useless sinecures, are not so numerous nor so large as to constitute a menace to society. Contrariwise, they should be encouraged and made more general, and be more effectively carried out. At the same time the attitude of mendicancy is not favorable to manly independence or intellectual vigor, either on heirs of the blood or on institutions (unless scientific research be an exception). Philanthropic aims may be rendered more certain either by concentration into systematic method, or by oversight and positive regulation from an exterior authority. It is a hopeful sign that the endowments, formerly going exclusively to churches and missionary efforts and the like, are now going more and more to technical and scientific institutions.

Fifth.—I need not discuss here the power, or the wisdom, of the taxing of legacies by the State; it is here, and promises to remain and grow. The only question for the sociologist now is: Shall the very considerable revenues derived from these sources be mingled with the general treasury and be expended for any or all State purposes, as in New York and many States; or, shall an effort be made to segregate this money and dedicate it to special purposes of benevolence, charity, and embellishment, thus to invite and, if wisely carried out, to win in advance the consent of the owners of large properties? I believe it is in the power of law, and within the compass of economic skill, to provide a residuary legatee for every such person superior to any other he could choose or contrive for himself, so that, in fact, it would practically make no difference whether he executed a will or not; or, if there were any difference, that the machinery provided by the State would be more sure to meet his views than any he could select. Nothing would prevent him from distributing his estate during life, if he felt so inclined; but on the other hand a portion of all beyond what was necessary to a competence to infants, to the disabled or incompetent, would be turned over to a Department of Beneficence, managed by skilled men as are our State Universities and the Smithsonian Institute, who should organize and methodize the philanthropic impulses of the rich with the ample revenues at command much better than the hap-hazard litigious course now open to them.

Sixth.—The same agency thus entrusted with the dispensing of voluntary as well as involuntary bequests, might be availed

of to do away with much of the necessity for hoarding or amassing riches, and to strip from great wealth some of the dangerous, or mischievous homage paid to it, which is the cause of so much friction and clashing in lives that otherwise might be serene. Annuities might be granted for the life of the purchaser or for the life of others, objects of his care and solicitude, and thus help to banish in large measure, the fear of the wolf at the door. The vicissitudes of business and health, the disappearance or loss of friends, the impatience of consanguines with enfeebled dotards, are all real causes for morbid acquisitiveness and these might be banished by a pension or competency for life. Steady, though moderate income, is conducive to longevity, sanity and tranquillity.

By such means the benefactor who is frequently at a loss what to do with his possession could have the double benefit of using it up, or all that was of any real use to him, and at the same time of seeing the remainder of it devoted (without the scandalous shrinkage which now attaches to many of our pet charitable societies) to the erection and conduct of hospitals, asylums, reformatories, libraries, Carnegie and Cooper Institutes, memorial statues and edifices, public parks and promenades, music halls and pavilions. I hold it certain that the most avaricious of heirs would sooner see the contested wealth expended in such a judicious manner than on court costs, lawyers' fees, and the like, or than given to the opposite side. If it be objected that inheritance taxes will be evaded by gifts during life, it is easy to see that this is one of the objects aimed at — to coerce rich men and women to dispose of their excess beyond the line of competence, and to see the good or ill it works and not leave this task wholly to surviving society. There ought to be a narrower limit drawn around the power of the dead hand. Because of the sagacity to amass a fortune, there is no reason in nature why that fact should carry with it after death an authority greater than to the living.

VI. THE ROLE OF SUPERSTITIONS.

Here let me say, we speak commonly of the sciences as if they were so many separate and independent fields of work, with distinct boundaries between them; and the division of these studies into so many different Chairs in our universities, and our own classification under ten or twelve groups or "sections" tends to confirm this supposition. It should be understood that Science is

systematic knowledge, and the pursuit of knowledge according to methods admitting of demonstration or reasonable certainty; and that each portion is not only interlaced with its near neighbor, but all are more or less blended and interdependent, so that any great advance in one affects the others, and, of course, any backwardness keeps back the near-related parts. The old conception of Science as a pyramid, whereof languages, mathematics and geometry were the basis and philosophy and theology the apex, we must now disavow; nor is that of a circle where the several sections could be evenly joined together or taken apart; or the conception of a linear prolongation as of a tree, or vine, having roots, stem, branches and twigs a true image. They are merely convenient symbols, but if accepted too strictly, they are apt to mislead. The present classification, like many it has displaced, is provisional and subject to re-formation as a whole, as are also the component parts. While mathematics and geometry remain substantially on the bases laid out in the infancy of science, other studies, such as astronomy, geology, physics, chemistry and biology, have been completely re-cast; nor ought we to suppose that a finality has been reached; indeed, there are signs that portend a reconstruction to bring them all into more harmony. From his psychical, purposive and coöperative nature, man himself must always be entitled to a large and somewhat exclusive space in scientific labors in any scheme of science. Social science has not yet had its methodical re-construction. Occupied, as it must be, largely with men's wants, motives and passions, it is essentially psychic, and awaits the evolution of the New Psychology — just now renascent— before it can claim to be a science of precision and prevision. Meantime, there is much to be done; the very terminology and nomenclature, framed on the old traditional theories, will require to be abandoned or charged with new meaning to fit them to new conceptions.

It begins to look as though the wrong road had been taken in psychology, and that very much of the confusion, clashing and misery of the world is traceable to the error. It does no good, for instance, but harm, to assume as the text books do, or did, that mankind alone exemplifies the social faculty; that he alone is capable of mental development; to deny a mind or soul to the other animals, when they exhibit such human emotions as affection, fear, anger, grief, jealousy, gratitude, avarice, shame, deceit,

coquetry, feigning, dreaming,—attributes proceeding, so far as they go, on lines parallel to those traversed by all men in the primitive stages and by some men up to this day, but whom on account of their backwardness we call savages. They are "wild men" as distinguished from the long domesticated man. As Huxley well said: "It was a great day for humanity when man succeeded in taming the canine brother of the wolf into his companion and servant, so that, instead of devouring the flock, he became its protector." [1]

In order that men and animals should survive the inclemencies of the elements, the encroachment of plant and parasitic life, and the attacks of carnivora, there must have been furnished by the process of selection, or by invention, the needed shields, and the weapons to counteract these destructive forces, with sufficient peace and order among their own kind to ensure the rearing of offspring, or otherwise the race must have long ago dwindled and perished. I need not go back over the several important stages in this progress upward, each marking a fresh control over natural forces: the bone implements succeeded by stone, earthenware, bronze and iron, followed by the prodigious array of mechanical inventions of the distinctly historical periods. Along with this improvement of man's environment and by its aid, there has gone an unfolding of mind faculties, an astonishing amplification of that function of the nervous and cerebral ganglia, quite in contrast with the growth of function of any of the other sense organs of the body, which has made possible an immense and complex use of symbols, and a psychical predominance which puts a gap between man and the other animals. These thinking powers and brain functions are, however, not exclusive to mankind; they are shared by the brutes, and by some birds, mammals and insects to a high degree. Take, for example, the remarkable intelligence of the beaver, the horse, the ant and bee, the magpie, which are sufficient to allow a high degree of forethought and of architectural skill, and which follow on the same lines as human thinking, so nearly as to disclose the germs of what in a man we style civilization, such as language in ruder form, organized warfare, defensive and offensive, the enslavement and domestication of captives, construction of store-

[1] Advantage to the dog and his escape from extinction followed. Sir John Lubbock has succeeded in teaching his dog to read; but for ages dogs have read the countenances and gestures of one another and of men just as children do, and with remarkable accuracy.

houses and barriers for future use, various social and coöperative actions, and we may even discern in some of their assemblages promptings and behavior very much resembling our deliberations or ceremonial worship. These feebler manifestations we have been accustomed to pass lightly over as instincts, falling short perhaps of reasoning, and whether or no, as lacking the special faculty of self-consciousness.

There is therefore a human and a sub-human psychology and (if mere aggregation or association of effort and action is to be accepted as a distinguishing line of division) a sub-human sociology also, so nearly related that continual reference may be made from one to the other as in the case of Comparative Anatomy and Pathology. For that wide segment of psychic activities peculiar to man, in which there is not merely a consciousness of likeness, of community of burdens and pleasures, but also social purposes, aims and organized or systematic social efforts, in which primitive society is conspicuous (individualism being a later growth), combining plans for security, comfort, enjoyment, aspirations, opinions, creeds, culture, interdependent arts and industries, we have no better name than civilization. That I take to be the cement which binds together individuals in a relation external to that of consanguinity or ethnic derivation. There are archaic types of this culture existing contemporaneously with the modern advanced ones, the strata being curiously intermixed and overlapping, barbaric traits cropping out everywhere upon civilization. The most convenient line of separation I can think of between them for the purposes of specialty subdivision, is that of literature, the writing faculty being the earmark of the civilized, and the lack of it of savage, peoples.[1]

[1] If the present classification is to stand, Anthropology covers all human affairs and thoughts — a field too vast for a single mind to master in all its detail. Ethnology, Archæology, Somatology and spoken languages would seem to belong to it somewhat exclusively. How then are the remaining human activities to be divided, such as civics, politics, demography (vital statistics), economics, ethics, esthetics, arts, industries, education, creeds, opinions, folklore, linguistics? Dr. McGee, in a very commendable scheme of Anthropology, has used the word Sophiology as a generic title for the more psychic of these, which seems to me to be a more scientific and more natural line of cleavage (and one which will appeal to the sense of responsibility of the Council) than Sociology — a hybrid at best, which not even the brilliant sponsorship of Comte, Spencer, Giddings, Small and others in professorial Chairs can reconcile with other sciences. Any arbitrary line of separation is attended with difficulty; but if these studies could be divided into two or three departments, it would tend to reflect the present trend of research.

In speaking of the passion for fighting, you will notice I endeavored to account for it as being derived mainly from ancestral experience, and as a trait common to man and the lower animals. We make no difficulty about accepting this explanation of those exhibitions of the mind faculty we call instincts; but in regard to the higher faculties such as reason, will, self-consciousness and the like, the philosophers have hitherto followed a different but not very satisfactory explanation, and have sought to reserve them to man alone. It is only within the recollection of those now living that any attempt was made to study the mind with the same freedom as any other phenomenon, and to reduce it to an experimental and comparative method; that is to say, for upwards of two thousand years, the notion of the duality of man had held the field and any profane meddling with the soul, as the invisible self, ego, or personality was deemed sacrilege, and would have met with ecclesiastical censure or penalties. The idea that the soul was something more than a function of the organism and appurtenance to it, was *per se* an entity, lawless, independent, directing, initiating and controlling the bodily movements, and capable of maintaining a varying struggle with the passions, emotions, instincts and intellect, was probably derived from Hindostan (although the doctrine of metempsychosis or successive bodily tenancy by the same immortal soul was somehow dropped out in the transit) about the time of Socrates and Plato, and to them and its engrafting on the Jewish mundane religion, we may attribute this long fencing off of mental phenomena from scientific observation. The plodding German metaphysics has done much to befog and encourage this cult of separable states of the mind and personality.

There are other traits besides the combative to be traced back to this race experience, such as the awe of the dead, or death-bed and burial, the sense of duty or "oughtness," the propensity for lying and stealing styled "original sin," the feeling of premonitions, and of a previous existence, the craving to penetrate the future. Out of these ingrained mind-traits, metaphysicians have sought to construct "forms of the understanding," "categorical imperative," and innate ideas, dictates of conscience, and from these various systems of philosophy and ethics. Indeed the claim is still maintained that there are two kinds of truth, the intellectual and the spiritual, and wherever their conclusions differ, the latter is entitled to superior validity. Confusion is thus intro-

duced; and we have the further claim of lifting faith above knowledge, and that religion and its bases have a sphere of their own from which science is excluded. This claim scientists have not taken pains to deny, for science need not hurry, but it concerns them deeply to resent it. While the spiritual theory is defective, I am not disposed to accept the more recent one of the genesis of religious ideas from dreams, ghosts or ancestor worship. These are supporting after-thoughts. The natural wonder as to the first breath of the newly born, the last breath of the dying, the heart-throb, ecstacy, suspense of consciousness, decay of mental powers, sportive variations have helped fasten the notion of a lawless and independent double or shade which, once accepted, has led to all manner of excrescent amplifications.

Underlying this guessing at the mysteries of life, the whence and the whither, the hôw and the why of it, we may trace alike in man and in brute an impulse of mind or instinct which is phŷlogenetic, rather than ontogenetic; it belongs to the chain of descent rather than to the last individual links, in which we may discern the germ of all religious emotion. It is analogous with the embryonic progression through lower forms of structure. I mean the inborn propensity of making-believe, of personification, and of poetizing in the original signification. This is the true fetishistic basis and bias; it extends to things animate and inanimate, especially if they have some semblance of animate forms. Doll-inventing and pet-loving are early exhibitions of it, and symbolism in art, image worship and ritualism are later ones. The inherent tendency to apostrophize, to endow or discover life or character, and to reverence is common to savages and to the children of the civilized, and is slowly outgrown by the former; while it lingers in poetry, folk-lore and superstitious observances among the latter. Essentially an emotional and feminine trait, it fades with experience and the light of reason; it is probably a vestige of the period when vocal utterance was but feeble and crude and when ideas were conveyed chiefly by signs, mimicry and gesture; the gestures of obeisance, supplication and tenderness being then, as now, very impressive. Such is, I believe, the origin of worship. It has a close analogy in the palimpsest-like reappearance of old and forgotten shadows on the sensitive plate of photography after they were supposed to have been erased and then overlaid with more recent films. Religious ideas had a large use and significance in the

infancy of the race and will continually crop out in spite of the culture of later time. In this respect the venerating or worshipping impulse may be likened to the sensation experienced by dramatic recitals, the excitement of the dance, or the soothing of rhythmic sounds, the exhilaration of rapid motion; the instinctive clutch of the infant which cannot have experienced a fall; the lingering passion for fishing and hunting; or, to take an extreme illustration, the excited aversion of a kitten at the first sight of a dog, or at the first smell of a mouse. By the light we now have we are permitted to offer a physiological explanation of phenomena of mind heretofore quite mysterious. Such puzzling phenomena as hypnosis, fascination, coincident cogitations, double and alternating consciousness are to be solved in this way.

The time was, not so long ago, when the physiologist would hardly entertain the soul as a subject of speculation, not even in disease, insanity being accounted as possession by devils. The physicist would have thrown it out of his laboratory window, if possible. The labors of anthropologists helped greatly to lead out of that old rut. The study of mind in the multitude, of the savage, of race traits, of mythologies, folk-lore and religious ceremonies betrayed their true origin, and disclosed the likeness of origin and especially the regularity of sequences we call "laws" of thought. The biologists still further broadened the base of psychology by tracing it down through the gradations of organization to the cell, and into plant life; nor is it certain that it stops with life, as we at present define life, but seems to be coextensive with organization.[1]

Science does not, in one sense, concern itself with teleological suppositions; that is to say, it is reluctant to resort to any of

[1] For opinions on the coextensiveness of mind with life and organism see Haeckel's "Creed," E. D. Cope's "Primary Factors of Evolution" (concluding chapter), Darwin's Letters, Clodd's "Pioneers of Evolution," Romanes and others, Neo Lamarkians and Neo Darwinians.

For expressions on the pervading reach of mind, as exhibited in unorganized matter—as a property of matter—in the curious behavior of substances like camphor-grains, gamboge, frost crystals on plate glass, figures on vibrating membranes, see current literature of Physics and Chemistry, Prof. A. E. Dolbear's "Matter, Ether and Motion," also the concluding sentence of his Lecture to Wood's Holl Biological Station: "At any rate it is evident that if any such theory of matter as is here presented be true, and if the behavior of matter as we see it in the test tube and microscopic slide has been interpreted with any approach to the truth, then it is a much more wonderful thing than the old philosopher's thought; its possibilities greatly exceed what could before have been imagined; and if mind itself requires a material habitat then it has in an atom an imperishable living home."

them to explain the observed cosmos, and prefers to listen in a neutral attitude to the rival philosophies: theism, manicheism, atheism, monism, spiritism, or materialism; but it is at least equally well equipped to pass judgment on any such speculations as their advocates. The attempt to waive students off from the domain of the soul or from religious beliefs and observances, is likely to be disregarded. In fact the educated ecclesiastic mind now anxiously awaits the verdict of science on the nature of mind, and the origin of some of these innate ideas. When it is found that they are naturally accounted for as belonging to the experience of the race, something not to be ashamed of and still less to be proud of, and are not likely to be superseded very soon, the doctrine will be welcomed as one averting a threatened dissolution. So long as the perpetuity and acceptance of the church dogmas were based upon the inspiration of certain writings, upon miracles, upon special providences, soul-peril and soul-rescue, or future rewards and punishments, it was exposed to the double risk and cross fire of successful contradiction by science, and by historical criticism, and also liable to the ridicule of scoffers and the charge of outrageous fraud. Science is now on the eve of supplying a broader and more enduring, or at least less precarious, basis for religious ideas than its votaries. When Pope Pio Nono was told that science was undermining some of the foundations of the church, he exhibited a profounder knowledge of human nature and soul nature than the casuists by his reply: "Then we must revive more relics and shrines."

Dr. Andrew D. White has rendered a special service to scientific research and also to ecclesiastical training by his compilation, with abundant citation of textual proof, of the successive awkward retreats from this position of superior spiritual discernment. Whenever the psychologists are ready with the lacking capstone of experimental observable proof as to the act of thought, the further measurements of its velocities, and transformations of matter or motion into that we call consciousness, they will be listened to with profound attention. No propaganda is needed to enforce the conclusions of science, the intelligible statement is enough to overcome the most unscrupulous opposition. What is now waited for is a psychic spectrum to throw, as upon a screen, the analysis of mental phenomena and to give the equivalent of Frauenhofer's lines in perception, memory, comparison, association, ratiocination, the several emotions below and above the limits of conscious-

ness, and especially to picture for us those puzzling sub-conscious states the mirages of the will and the self-examination. We may confidently expect (probably from another rash layman) some form of mental bolometer which shall record for us the dimensions and count the number of the vibrations of this hitherto unsuspected mode of motion, if not also to disclose those delicate films which carry some but not others of those external stimuli down to posterity, and fix the stamp of ancient habits, passions and character long after their necessity or utility has vanished. Is it any more wonderful than that the two forces of heredity and environment should preserve the bodily forms and organic structure for a like period? After the publication of Haeckel's famous work demonstrating that man himself in embryo, and often in life, bears the evidence of derivation from, and similarity of nature with, the lower animals, it is not incredible that these mental traits should accompany them for the latter part of the period. As passing from mother to child, we accept them along with the physical features unquestioned. We easily detect them for three or four generations. Why not for ten thousand composite impressions? One is tempted to use the exclamation of Huxley after his first reading of the "Origin of Species:" "Why, how stupid of us that we never thought of that before!"

The retrospect extending so far back enables science to look forward a trifle. The decay of faith, and the crumbling of dogma already giving anxiety to thoughtful men within the ecclesiastical pale, is giving rise to the question: "What is to become of morality when its supernatural sanction is lost?" Prof. Goldwin Smith, from the historian's point of view, naturally apprehends a serious jolt, and doubts whether the present structure of society can stand the transition. The view here taken may reassure the timid. The doctrines which are causing such naïve embarrassment: the fall of man, the atonement, the resurrection of the body, the real presence, anthropomorphic deity and filiation, angels, devils, future rewards and punishments may fade out and disappear — may in fact be relegated to the region of extinct mythologies—but the underlying religious idea, the worshipping instinct, will remain. It is as perennial as the belief in fairy tales, nuptial-revels, serpent-fear, omens, portents, myth-making and ceremonial charms, amulets, lucky stones, superstitions; and having the same ingrained origin. They are vestigial experiences of the race—an inheritance to be outgrown, but meanwhile costly impediments.

VII. DIVERSITY OF LANGUAGES.

That type of civilization cannot be regarded as ideal or forethoughtful which tolerates a wide diversity of tongues in which to conduct its business or store up its ideas and valuable records. As already stated, difference of speech and writing tends to keep nations and races estranged, and so makes for war rather than peace. The only progress toward a uniformity of mother tongues now visible, is by the slow and fitful process of political absorption by conquest or by trade. Singularly enough the acknowledged languages of learning, the Latin and Greek, seem to be losing rather than gaining their hold upon the best literature. This is not altogether a misfortune; for languages grow and expand to conform to the ideas of those who use them; and the original connotations of words are lost in their adaptations to new conceptions. In spite of the attempt to uphold the Roman tongue by the medical and priestly professions, it is no longer that spoken by Cicero. The English of to-day differs widely from that of Chaucer. But few famous treatises in science, philosophy, history or even theology, are now written in Latin; other tongues command more readers, and it no longer so well serves as a vehicle for modern ideas. No language can escape this fate. The English, which is conceded by competent observers to be as rich, as flexible and precise as any of the great European tongues—though not as simple and symmetrical as some others—has embalmed in it quite as many of the indispensable works of the world, and has besides the suffrages of a hundred and twenty millions of people to whom it is vernacular, is nevertheless susceptible of great rectification, especially in the matter of pronunciation, spelling, and in the irregularity of the verbs. The testimony of Professor Merz, in writing of "Scientific Thought in the 19th Century," although strangely oblivious of American contributions, as such, uses the following language, after referring to the decaying use of the classics:[1]

[1] "The largest number of (Scientific) works perfect in form and substance, classical for all time, belongs probably to France; the greatest bulk of scientific work probably to Germany, but of the new ideas which during the century have fructified science the larger share belongs probably to England. Such seems to be the impartial verdict of history. During the second half of the century, a process of equalization has gone on which has taken away something of the characteristic peculiarities of earlier time. The great problems of science and life are now everywhere attacked by similar methods. Scientific teaching proceeds on similar line, and ideas and

Nevertheless the hope of establishing either Latin or Greek as alternative world-languages, of learning, has not been abandoned among the classically educated; but all expectation of seeing the former generally adopted, at least as a spoken tongue, must have passed. If the great start of the Roman empire, and the subsequent extension of its speech over a larger empire by the church, did not suffice to give it precedence the chances are much against it now. Like the Roman jurisprudence it lives chiefly in its offspring. It has been more or less engrafted on the native tongues; itself is practically a dead language. The Greek survives among living tongues, but has only a limited field as such. In scientific and classical education and notably in nomenclature, it has a future of utility as an enricher. Some of the international medical conferences are, I believe, ready to adopt it as an alternative language for their limited uses.

Meantime the business of the world becomes more and more international and interlingual. The spread of telegraphs by land and under seas, the extension of steamships and steam railways across frontiers, sometimes across several of them, not only crowd the nations together but some common code of communication between them is a desideratum—the world of commerce no less than that of letters and research waits for it. Regulations for navigation on the high seas have been contrived by the maritime nations and adapted to all; we have likewise a growing communication and conformity in astronomical, chemical and electrical literature; uniformity of standards of weight and measurement, mechanical devices and the like. In a small way too we have a universal language in musical notation; in the telegraphic alphabet, in the deaf-mute and in algebraic signs. How much longer will the international requirements of the whole world have to wait before a real world-language is hit upon? Must we wait until the struggle for political boundaries of the dozen or twenty several nations of Europe has concentrated the smaller ones into one dominant prodigy? If not, when and how shall the movement be begun and carried out, and by whom? The time seems to be ripe for a practical consideration of these questions, and it concerns some association of learning to do so; and

discoveries are cosmopolitan property. So much more interesting must it be for those who have been born members of this international republic of learning to trace the way in which this confederation has grown up, what have been the different national contributions to its formation, and how the spirit of exact science, once domiciled only in Paris, has gradually spread into all countries and leavened the thought and literature of the world."

for several reasons the initiative would seem to be left with the Department of Social and Economic Science.

I need not enlarge upon the magnitude of the continuing loss from the present diversity of tongues, not only in the time and effort spent in acquiring several languages, when one beside the vulgar tongue might answer all purposes of education, if that other alternate tongue were common to the great civilized nations. The waste is still greater from the publication of researches, laws, treaties and records in several dresses, all of which must be consulted by the student who would keep abreast of the advance of knowledge. The shelves of our libraries are being piled high with books of all shades of usefulness and uselessness, and an extensive ransacking of bibliographies is required to master any given topic. The most of these have only ephemeral value, but this again adds to the burden. One good effect of an alternate language of learning would be the saving from this weary plowing of the sands; the truly classic works worth preserving would in a few generations be winnowed out and a lifetime would not be consumed in mastering the works of authors long superseded, but which, as they now stand mingled side by side, are indistinguishable. An *Index Expurgatorius*, by a scientific college *de propaganda fide*, is not in accord with modern notions, but it would be a great step in advance to have all science uttered in one language and reviewed in the same. When one thinks of the ten thousand volumes printed annually by the presses in English alone, one is tempted to sympathize with that Arabian calif who ordered the great library of his time destroyed on the ground that it was either superfluous or heretical.

Observe, there is no suggestion to invent a new language such as Volapuk aspired to be. We all know languages grow by laws of their own, and are not run into a mold. They are, however, plastic and susceptible of enrichment and improvement by human contrivance. Instances are quite numerous where one tongue has supplanted another; and the example of two or more languages being taught and used concurrently is quite common. In fact, the task of imposing a second speech on a nation is much easier than that of imposing another religious cult, or a change of metallic money standards either of which is still deemed to be feasible.

The growth of languages may be compared to the formation of common paths and roads through the primitive wilderness; at first following the trails of the wild beasts; whenever a tree falls across

the path it is deflected and so continues long after the obstruction has crumbled away. The tendency to these deviations and doublings seems to be inherent. The French is about the only tongue which has an officially appointed guardian to keep it within orthodox lines; and it must be added that none needed it so much, or has so much to be done for it remaining. What is needed, and would seem to be practicable, is the application of modern methods equivalent to the work of the civil engineer among the time-worn paths—a levelling and alignment, the taking out of kinks and détours, and introducing greater precision and definiteness. It is no greater task for our time than the change to the Julian calendar was for that, and is comparable with the slow spread of the Arabic alphabet and numerals, displacing others, and vastly more economical than the proposition to divide the year into 13 months of 28 days. I fear it is not the proper or congenial rôle for philologists and lexicographers whose task will come in at a later stage, in the perfecting and grafting upon the adopted alternate language. Thus far their special interest seems to lie in the diversity rather than in the uniformity of tongues; and their very modest efforts to introduce a more regular spelling and pronunciation, though not entirely barren, are by no fault of theirs, hopelessly slow of adoption. The chances of these reforms would be better if English could be adopted as a world language; and if another were chosen they might be needless.

This Association is called upon from time to time to join in International Conferences, to recommend or appoint delegates to such gatherings, and to pass upon their reports touching matters of nomenclature, classification and standards. The cause of learning has very much at stake in an extension of this same function to language. Other interests are also concerned, and whether these other interests—foreign commerce, diplomacy, or telegraph or transportation—shall take the initiative, or leave it to others, there should be a joint action and representation. This subject is already attracting the attention of practical business men who may be expected to move in the matter faster than the teachers and lexicographers. While writing, my attention has been called to an address by a business man to a Boston business club, advocating the use of English as a world language. A table quoted from Mulhall, showing the growth of the great European language in the years 1801 to 1890, shows that the English has increased 217 per cent, while no other except the German has reached so much as 100. I have added to

it a column of estimated numbers using the same tongues at the present time, in which the lead seems to be with the English, though this is liable to be challenged by the partisans of Russia, as the official language, though not the native one, of a nearly equal number.[1]

Can we assume that this lead can be maintained for another century, when the Russian empire shall touch the two oceans and the Mediterranean, or when the German empire shall extend from the North Sea to the Bosphorus? If not there is nothing to be lost, and much to be gained, for us, by an earlier rather than a later settlement of this question.

There have also appeared in the daily press expressions from some learned society of Germany, which I am expecting to see authenticated any day, a willingness on their part to adopt the English as an alternate world language, provided some necessary reforms were made in spelling and orally to make it more phonetic and conformed to the classic Latin and Greek. This is a very reasonable and fit concession to be imposed, and ought to be undertaken in our own behalf without regard to the propaganda. If, by some such concessions as these, the support of Germany and perhaps also Holland, Scandinavia and Spain, can be won, the adoption of the English is assured; and we cannot too soon convene an International Conference. The Germans are handicapped by a Gothic eye-destroying alphabet, and an unmusical vocal speech, and are conscious of it. This is their opportunity and ours. The claim of the French as the established language of diplomacy is recognized in Europe, but declining even there, would be outweighed even though supported by Russia. Opposition would be likely to come from that quarter, if from any; or from a possible coalition of all the rest against the leader. But

[1] INTERLINGUAL CONFERENCE.

Mulhall's Table of increase, 1801–1890. (% in 1801.)	(% in 1890.)	Millions spoken by in 1895. (Estimated.)	
12.7	27.7	English	120.
19.4	12.7	French	46.
18.7	18.7	German	87.
9.3	8.3	Italian	32.
16.2	10.7	Spanish	22.
4.7	3.2	Portuguese	15.
19.	18.7	Russian (?)	129.
100	100		
		Scandinavian	9.
		Holland	5.

fortunately this is a case in which there is no compulsion. No nation need be bound by any recommendation of the Conference, if it thought it could do better to stand out. In brief it is the counterpart of the decimal metrical system; the advantages and drift of any action would be toward uniformity sooner or later. Professor Mahaffy is out in a very pronounced opinion as to the need of rectifying English; while Mr. Havelock Ellis I perceive is quoted as favoring French as a second choice.

My own idea about the manner of calling, and the composition of, such an Interlingual Conference is that, by virtue of her much greater foreign commerce, marine interests including telegraph, postal, consular, and diplomatic intercourse, the initiative would properly belong with the mother country. Any such call from her would be sure to suggest some antagonism and most likely also she would be asked to content herself with one vote on behalf of Britain and all her colonies; and attempt might be made to link in the United States. I have no idea that representation according to aggregate population would be acceptable. The most feasible plan will be by nations, or groups of nations, the offshoots and colonies not being reckoned except in the single case of the United States, which, if expedient, could speak for Canada too. The position of North America is one of peculiar freedom from jealousies and entanglements, and if the mother country will for this occasion graciously let her full-grown settled daughter appear in the foreground, there will be less friction to encounter and the result will be the same in either case.

There is a certain fitness aside from its expediency. American lexicographers and philologists have done more for the improvement of English in a hundred years than the British. Besides, the number of universities and students and the literary output are now comparable in volume if not in quality, with the older nation. The ultra-conservatism of British publishers is shown by an unwillingness to handle books by American authors using the abridged spelling of certain common words where the right of argument is on our side. Again, in Asia, especially in China and Japan, which are now open to Occidental literature, science and arts, we are side by side with the British and opposed by French and German influences. If I am rightly informed, Japan is most anxious for uniformity; in fact, would accept readily a common tongue, and prefer the English. The part to be played by these islanders of the far East in

international affairs cannot yet be defined, but their alliance in these bonds of peace, civilization and learning, is worth cultivating.

As a rough outline of the composition of the first conference on an alternate common language for international trade, intercourse, letters, science and arts, let us suppose that whenever a sufficient number of avowals of interest in the subject shall have been received from representative bodies, an invitation shall be addressed by the Secretary of State of the United States, or by this Association, or some similar body, to like associations and guilds in the following countries, to choose delegates to meet at some suitable time and place in Central Europe:

1. Great Britain, including colonies and India.
2. United States of America and Canada.
3. Germany not including Austria.
4. Austria and the Hungarian and adjacent Slav States.
5. France including her colonies and Belgium.
6. Spain and Portugal.
7. Italy.
8. Greece.
9. Holland.
10. Scandinavia (Denmark, Sweden and Norway).
11. Russia.
12. The Spanish and Portuguese republics of North and South America.
13. Japan (by courtesy, not voting).

Each of these units to be represented by, say, five delegates drawn respectively from the larger international interests.

A. Political, diplomatic and jurisprudence.
B. Scientific, mechanical and medical.
C. Foreign commerce and navigation.
D. Telegraphic, Foreign Exchange and Postal.
E. Pedagogy, Philology and publishing.

Here we may have a polyglot convention of say sixty-five persons, with sixty votes, representing various pursuits. All that it need do is to pass resolutions after preamble recommending to their respective governments, that it be made lawful on and after a certain date, say January 1, 1901; or as soon thereafter as may be, to use the language adopted, and that it shall be taught in all public schools as a second, or alternating, language; and further that all docu

ments for interlingual use such as passports, cable and telegraph blanks, navigation charts and astronomical codes, postage stamps, money orders, letters of credit, coins, tables of metric systems, shall be inscribed in both media. Similar action on the part of the guilds and institutions themselves would be sufficient to ensure the trial.

The work of simplifying the adopted tongue, so as to make it more acceptable and more easily acquired by the rest is quite another function, belonging to a different body, and can be reported on from year to year without limit of time. Our newest dictionaries contain already some thousand of minor and acceptable changes. It would greatly add to the regularity and euphony of the English (if it should be chosen) to incorporate and substitute freely from the Spanish as written (not however including the eccentricities of its pronunciation) in which case the Latin and Italian method should be taken ; in this way the good will of our neighbors on the American continent might be secured, with no detriment whatever to ourselves. Computations are sometimes made to show the enormous aggregate loss from the use of redundant or silent letters in writing and typesetting. This economy is easily embraced within the larger reform outlined above.

Elizabeth, New Jersey, U. S. A.

APPENDIX.

UNIFORMITY OF SCIENTIFIC AND OTHER LITERATURE.

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.
DETROIT MEETING, 1897.

The following preamble and resolution, originating with SECTION I — "Social and Economic Science," and duly reported by the Council to the Association, was adopted Aug. 12, 1897, with the recommendation that copies thereof be forwarded together with the explanatory remarks of Vice President Colburn to corresponding foreign Associations and Institutions of learning, for information in order to elicit responses looking toward the greater uniformity in scientific and other international literature, by an international conference or otherwise.

WHEREAS this Association is from time to time called upon to recommend or choose delegates to international conferences seeking to promote uniformity in scientific classification, nomenclature, metrology, publications, and is likewise interested in uniformity of navigation and postal regulations, and researches at present recorded in several differing European languages; and

WHEREAS the diversity of tongues is a continuing hindrance to interchange of knowledge and literatures, seriously enhancing the cost and labor of studious pursuits, which might in large measure be avoided by the adoption by the civilized nations of an Alternate Language of Learning, Law and Commerce, and as such required to be taught in higher schools (in combination with the mother tongue) and used in interlingual correspondence and printed records; and

WHEREAS it is believed this need is felt and acknowledged by societies and corporations of several nations and awaits the initiative of some one of them to propose concerted action thereon; now therefore be it

Resolved, that whenever the President or Permanent Secretary of the Association shall have received from similar bodies, or from Universities of Europe, sufficient in number to represent a majority of the maritime peoples, expressions signifying a desire to coöperate in an International Conference of Languages, it shall be his duty to lay the same before the Council at the next regular, or, if need be, at a specially-called, meeting, with a view to the appointment of one or more delegates to represent American Pedagogy and Science thereat, at some convenient time and place in Central Europe.

In like manner the Permanent Secretary is hereby authorized to acknowledge, on behalf of this Association, receipt of such invitation for a like purpose emanating from any government, or department thereof, Institution of Learning, Technical Science, Chamber of Commerce or Finance, Telegraphic or Transportation Bureau, Postal Union or Academy of Arts and Letters, and to pledge the further attention of this Council to the same.

PAPERS READ.

[ABSTRACTS.]

THE MUNICIPAL GOVERNMENT OF ONTARIO. By C. C. JAMES, M. A., Secretary of the Ontario Bureau of Industries, Toronto, Ont.

REFERENCES.

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2. The Ontario Township, by J. M. McEvoy, Toronto Univ. Studies in Political Science, 1st series, No. 1. 1889.
3. Reports of the Commission on Municipal Institutions appointed by the Government of Ontario. Toronto, 1889.
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5. Municipal Institutions, by C. R. W. Biggar, Q. C., in Canada Law Journal. Toronto, January 2, 1897.
6. Municipal Institutions in Canada, by C. R. W. Biggar, Q. C., in Handbook of Canada, Br. Assoc. Committee. Toronto, 1897.
7. Reports of the Ontario Bureau of Industries. Toronto, 1886-96.
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THE province of Ontario was first settled by a large number of persons who came, principally from New York state in 1783 and following years. The first legislature of Upper Canada was convened in September, 1792. In the following year legislation was enacted providing for local government through Quarter Sessions. In 1841 District Councils were introduced. In 1849 the Baldwin Act provided for township and county councils. This has remained up to the present as the foundation of municipal government in Ontario, except that in 1896 an act was passed whereby the number of members of County Councils has been greatly decreased. The basis of municipal government in Ontario is in the township rather than in the county, thus following the New England system rather than that of Virginia. The Crown Lands Department of Ontario gives the area of Ontario as 126,000,000 acres, and the incorporated portion is made up as follows:

1895.	AREA IN ACRES.	ASSESSED POPULATION.
492 townships	23,114,356	1,109,631
137 villages	100,943	136,021
96 towns	146,308	295,523
13 cities	40,548	416,215
Total,	23,402,155	1,957,390

It will be seen, therefore, that over 80% of the province is unorganized. In this unorganized portion there are at present 161 surveyed townships open to settlement. There is, however, an immense portion unsurveyed and a very large portion even unexplored. The census population in 1891 was 2,114,321. Townships vary in size and form in the older sections; the latest surveys are squares, six miles by six miles. Township incorporation may take place when there are 100 resident freeholders, and a group of townships may be incorporated as a county when the total population amounts to 17,000. A village may be incorporated when 750 persons are resident within an area of 500 acres or less. The village may become a town on attaining a population of 2,000, and the town may become a city when the population reaches 15,000. Sometimes, when the population does not reach the amounts given above, incorporation takes place by a special Act of the Legislature—seven of the cities for instance having less than 15,000 were thus incorporated. Every municipality is controlled by a council elected by the ratepayers. The head of a township or village is a reeve, of a town or city is a mayor, and of a county is a warden. The powers of these councils are limited and regulated by the Legislature of the province. The schools are controlled by boards of trustees, also elected by the ratepayers, but these boards are dependent upon the councils for their grants. Previous to 1897 the county council was composed of the reeves and deputy reeves of the various townships, towns and villages composing the county, but by Act of 1896 the councils are now composed of from 12 to 18 members elected separately, two for each district. Township matters, such as roads, bridges and public schools, are looked after by township councils; county matters, such as jails, poor houses and high schools, are looked after by county councils. In cities of over 100,000 provision is made for the appointment of a Board of Control, consisting of the mayor and three aldermen. This Board is a sort of Executive Committee of the Council, having control of the appointment of officials, awarding of contracts and the fixing of rate of taxation. Toronto alone comes under this regulation. The taxes of the county council are imposed by the township council, so that the rural ratepayer pays in one bill his school tax, his township tax and his county tax. A Provincial Municipal Auditor has been appointed as an official of the government to examine and audit municipal officers' books and to prescribe uniform and satisfactory systems of keeping accounts. All municipal officers are required to send reports yearly to the Ontario Bureau of Industries, which publishes the

same in tabulated form. The following is the summing up of the financial statements of all the Ontario municipalities for the ten years, 1886 to 1895:

Year.	Assessed population.	Total assessment	Taxes imposed for all purposes.			Bonded debt.		Floating debt.	Interest paid on loans and debentures.
			Total.	Rate pr head.	Mills on doll.	Total.	Rate per head.		
		$	$	$ c.		$	$ c.	$	$
1895......	1,957,390	821,466,166	12,316,429	6 29	14 99	51,895,991	26 51	5,834,129	2,578,220
1894......	1,936,219	826,179,370	12,320,312	6 36	14 91	49,724,587	25 68	9,669,567	2,552,607
1893......	1,910,059	825,530,052	12,512,060	6 56	15 17	48,083,243	25 17	6,796,422	2,508,621
1892......	1,909,527	825,211,127	11,803,570	6 18	14 30	47,166,962	24 70	6,469,890	2,482,156
1891......	1,922,121	818,847,394	11,767,748	6 12	14 37	43,889,853	22 83	7,629,730	2,498,294
1890......	1,917,344	798,616,271	10,897,485	5 68	13 65	40,720,985	21 24	8,387,186	2,240,692
1889......	1,906,001	761,995,816	10,248,198	5 37	13 45	38,988,332	20 44	6,493,519	2,057,998
1888......	1,880,145	748,654,570	9,919,962	5 28	13 25	34,729,527	18 47	6,437,393	1,909,760
1887......	1,848,457	717,311,988	9,300,113	5 03	12 97	31,943,320	17 28	5,645,208	1,820,590
1886......	1,828,495	694,380,659	9,009,385	4 93	12 97	29,924,863	16 37	4,841,717	1,715,620

CIVIL SERVICE REFORM: (1) CONFLICT WITH THE SPOILS SYSTEM IN THE STATE OF NEW YORK. (2) RELATION OF THE SYSTEM TO THE QUESTION OF STATE AND MUNICIPAL OWNERSHIP OF QUASI-PUBLIC WORKS. By WM. H. HALE, Ph.D., Brooklyn, N. Y.

THE first part of the paper gives a brief account of the conflict of the civil service reform with the spoils system in the State of New York which culminated last spring in the decision of the Court of Appeals in the leading case of Chittenden *et al. vs.* the Mayor of Brooklyn, *et al.*, and the passage of the anti-reform bill by the legislature. The second part of the paper discusses the question of the bearing of the civil service reform system on the practical operation of public franchises in the nature of monopolies, such as water supply, gas and electric lighting, telegraphing and telephoning and street railways by the municipality; and advocates the system as being well adapted on the one hand to secure competent employés, and on the other to check the overgrowth of political patronage and corruption. There are many obvious advantages in having a responsible government control and operate all these franchises which use public highways, and supply public necessities. It is better that when streets are torn up and repaired, it be by those who are interested in keeping them in good order. It is well that the comfort and convenience of the people who are supplied by these monopolies be made the paramount consideration, rather than the gain of a selfish corporation. But work of this kind requires skill, integrity, industry and experience, which can only be secured by a careful selection of men for

merit and fitness, and the best men cannot be obtained without a somewhat stable tenure of office. To leave these difficult and important public works a spoil to politicians would seriously impair their efficiency, and would also confer upon party leaders a dangerous power of irresponsible and unlimited patronage. The system of reformed civil service which aims to secure the best men for the performance of public duties affords the only economical and therefore the only practicable solution of the problem of governmental administration of these natural monopolies for the public good.

Racial deterioration: the increase of suicide. By Lawrence Irwell, Buffalo, N. Y.

Contributions to the development of meteorology by the Smithsonian Institution. By Dr. Marcus Benjamin, U. S. National Museum, Washington, D. C.

This paper is an historical summary of the development of the meteorological work of the United States as fostered under the auspices of the Smithsonian Institution, and includes the description of the first weather map as well as mention of the beginnings of the application of the telegraph to the collection of conditions of the weather.

[This paper is printed in "The Smithsonian Institution, 1846–1896: The History of its First Half Century."]

The economic position of women. By Miss Mary Forster, Buffalo, N. Y.

The position of women wage workers is still a story of long hours, low pay and, in the case of married women, household work in addition. Charity, even when organized, is the worst method of attempting to better their condition. It has been difficult for women to organize into effective unions. Legislation, in the direction of Factory Acts, is the best immediate remedy. The educated working women are less helpless, because less ignorant, and not a prey to the sweater, but have to contend with more direct competition with men, to take low salaries, and to suffer from the

competition of the partially supported and the gratuitous worker. It is the working women of this class to which our conservative friends object; but the objections they make would apply equally well to all women and they endure complaisantly the condition of the women wage-workers, regardless of the injury their sufferings cause to the race. Educated women have been forced into the competition for the means of subsistence, by the way the present industrial system has developed. This has brought about a great change in their attitude towards society and in their aspirations. Finding themselves no longer dependent beings, they cease to limit themselves to the narrow ideal man has had with regard to them. In the interest of their sex, and for the ennobling of the race, economic independence is their demand.

THE COMPETITION OF GRATUITOUS WORKERS. By Miss MARY FORSTER, Buffalo, N. Y.

THERE are many employments where the worker finds his worst rival in those fairly well off, who are willing to work for less wage merely to obtain extra comforts or luxuries. Among the poorer classes of women workers, homework, as opposed to work in factories, has not only been the real cause of sweating, but in many trades has admitted of married women and others who are supported, and have a few hours to spare, using them to undersell the regular employés. Factory Acts are of course the remedy for this. But how shall we prevent the ambitious or vain among the wealthy from gratuitously or cheaply supplying the public with literature and art? Newspapers are the mediums through which a great deal of this supplanting of the professional by the amateur is done. In many cities they are to a great extent written gratuitously, and as a result are ill written and unreliable. Our journalism is, in fact, largely pauperized. This injury to professional workers is more rife among women than men; and if those who give help gratuitously as typewriters, secretaries, teachers, etc., realized the economic pressure of the present industrial system on thousands of their sisters, they might refrain from supplanting them. That they should study social conditions is perhaps too much to expect, and it seems hard that, being wealthy and not compelled to do anything for the community in return, they should be debarred from all effort to make themselves useful. But from the professional side of this question, we have to consider possible remedies. (1) Legislation, which is impracticable except in cases where employments can be organized into regular professions, as doctors and lawyers are. (2) Combination, in guilds or unions of some kind, to bring pressure to bear on outsiders, as well as to improve the technical skill, knowledge and appliances of the members.

THE TRUE MEANING OF THE SUGAR SCHEDULE OF THE NEW TARIFF. By Prof. H. W. WILEY, Dept. of Agriculture, Washington, D. C.

THERE are three ways in which duty is levied on imported sugar: First, a duty according to the saccharine richness of the sugar as determined by the polariscope; second, a differential duty on all sugars above No. 16 Dutch standard of color; third, a countervailing duty on all sugars imported from countries which pay an export bounty. Under the first head, taking as a base raw sugar 75 per cent pure, the duty is 95 cents per 100 pounds. This duty is increased 3½ cents on each additional per cent of sugar, thus bringing the duty on pure sugar up to 182½ cents per 100. The Dutch standard is a case of 14 bottles filled with sugar of different colors, ranging from dark brown (No. 7) to almost pure white (No. 20). Under the second head, the tariff is 12½ cents on all sugar above No. 16, the division line in the tariff schedule. This No. 16 is a dirty yellow sugar, unfit for use on any of our tables. Practically, therefore, the differential duty of 12½ cents per hundred must be paid on all sugar imported that is fit for consumption. For pure refined sugar it would make the duty $1.95 per 100. Many European countries pay export bounty on sugar leaving their ports, notably Germany and France, the chief sugar-producing countries of Europe. For refined sugar, Germany pays an export bounty of 38 cents per 100 and France 39 cents. When refined sugar is imported from Germany and France, therefore, the amount of duty to be collected will be 195 cents per 100, plus 38 cents and 39 cents respectively for the two countries. This largely increased duty would exclude all these sugars from our markets were it not for the fact that by reason of the export bounty the actual purchasing price is diminished; so that upon the whole, by reason of the low price at which these sugars can be purchased, they may still be imported under the increased rate of duty. The annual consumption of sugar in the United States is now 2,000,000 tons annually, of which less than 300,000 tons are produced here. The annual importation, therefore, is about 1,700,000 tons. Before the civil war in Cuba, 1,000,000 tons annually were imported from that island, but since the war we have imported from there less than 200,000 tons. As a result, the amount of beet sugar imported has rapidly increased until it is now 800,000 tons per annum. It is evident that the secretary of the treasury will be placed in an embarrassing position when he levies for the first time against the friendly countries of France and Germany a countervailing duty on imports of sugar, and it will require a rare combination of diplomacy and expert ability to arrange these matters satisfactorily to all nations concerned. The degree of protection afforded the sugar trust by the new schedule has been widely discussed. It is difficult to determine the amount of it. The maximum protection possible would be 69 cents per 100 pounds, but that cannot be obtained in practice. The minimum protection would be 12½ cents, but this will always be exceeded in

practice. The actual protection will vary according to the character of the raw sugars imported. Inasmuch as the character of the raw sugars is largely under the control of our importers, it is fair to assume that the degree of protection will be the maximum consistent with ordinary commercial transactions.

THE U. S. IDEA IN LAYING OUT THE PUBLIC LANDS AND THE EVILS RESULTING THEREFROM. By BOLTON WALLER DE COURCY, Tacoma, Washington.

THE author assumes that, when Judge Oliver Phelps proposed the system adopted by the United States for the division of public lands, by north and south and east and west boundaries, he was actuated by the idea of giving the farmer the maximum of land, with a minimum of fencing. This, he argues, was an error and caused damages in various ways on account of slopes of greatest drainage and scour, and he suggests as a remedy the cultivating of the land on lines of least abrasion.

WHEAT CONSUMPTION IN THE UNITED STATES. By HENRY FARQUHAR, Department of Agriculture, Washington, D. C.

THE average amount of wheat eaten by an individual in the course of a year differs widely in different countries. At the head of the list is France, with a per capita consumption equaling or exceeding eight bushels; at the other extreme (among civilized peoples) are to be found Russia and Scandinavia, whose demands for wheat do not exceed from one to two and one-half bushels, the place of it being supplied by rye, oats and barley. The British colonies, Canada and Australasia, have a place near the head, followed closely by Great Britain herself, where the average for the last thirty years has been quite satisfactorily shown to vary between five and one-half bushels and a little over six, according to the cost of the flour. The place of Germany, on the contrary, is low on the list, its people being partial to rye bread. Where do the United States come in this series? Their true position has not yet been conclusively determined. Some estimators, of high capacity and care, have put the figures as high as five and one-third or five and two-fifths bushels per capita per annum; while others, not inferior in care or capacity, have made the rate as low as four bushels. The reason of this wide divergence is the difficulty of ascertaining the true amount, which difficulty results from the extent of the country and variations in crop-cultivation rendering the estimates doubtful, from the considerable difference among the different parts of the population in the degree to which corn-meal and

other substitutes replace flour, and from the uncertainty—not yet altogether removed—as to the relative number of those making great and little use of substitutes.

There are two methods of ascertaining the wheat consumption of a given population; first, by allowing, from the total wheat product (increased by importations of wheat and flour, when there are importations) for exported wheat and flour and for stocks used for seed (and for stocks fed to animals when there are such) the remainder being divided by the population; second, by actual account of the consumption of certain families taken as representative. It is plain that the first method must fail if we have not an accurate reckoning of the total product, and that the second must be misleading if we do not select cases truly representative, or if we allow weights to the different amounts consumed in different parts of the country, out of proportion to the number of consumers of those amounts. The National Department of Agriculture is now investigating the question according to the second method, and will shortly, it is hoped, be able to publish some definite results.

Labor restrictions as potent factors in social evolution. By Charles Porter Hart, M.D., Wyoming, O.

When we compare the present state of society with those of former periods of the world's history, we are at once struck with the fact that the former has been the result, to a very great extent, of the various agencies affecting labor; and that in proportion as the labor movement has been free and unrestricted, except as influenced by the operation of natural laws, not only the general happiness and welfare of the people, but the uniform development of all that tends to the best interests of society—such as education, progress in the arts and sciences, the moral and social elevation of the masses, etc.—has so uniformly kept pace with it, that we are forced to the conclusion that the labor movement constitutes one of the most potent factors in modern social evolution. This fact stands out most prominently, when we observe the influence on society produced by strikes among workmen, whether voluntary on their part or involuntary; that is, whether produced by the action of the natural laws of trade, manufactures and commerce, or by the force of social organization. In the former case, the disturbance, if any, is so gradual as to be scarcely discernible, while in the latter, they are seen and felt through every ramification of society, the capitalist suffering as well as the laborer, the consumer as well as the producer. The tramp problem is but a dependent branch of the labor movement, the vast army of tramps throughout the country being constantly, though irregularly, augmented by restrictions of the sort just referred to. This army serves as a kind of diverticulum, to receive a portion of the surplus labor of the country;

'itional evil falls upon the community, not only by the with-
s from their legitimate pursuits and by the direct results
n the various products of industry, but by the degra-
For this reason, more especially, I regard it as one
of the times, that an enlightened judge has just
enal offence, and injurious to society. This
t one, and will probably have a most salutary
en in preventing strikes among laborers. That
ital and labor are mutual and ought to work harmoni-
goes without saying; consequently, whatever tends to les-
tion between the two classes cannot but prove beneficial to

e above statements were supported by numerous facts and illustrations adduced by the author, most of which were the results of his personal observations among strikers and miners.

ANNUAL GROWTH OF FOREST TREES. By Prof. W. R. LAZENBY, Ohio State University, Columbus, Ohio.

WEIGHTS OF BEES AND THE LOADS THEY CARRY. By Prof. C. P. GILLETTE, of Agricultural College, Colorado.

A NOTE ON THE SILVER QUESTION. By ARCHIBALD BLUE, Toronto. Can.

THE world's production of gold and silver, computed in fine ounces, is presented in the following table by annual averages for groups of the first five years of each decade in the last half of this century:

YEARS.	GOLD.	SILVER.
1851–55	6,397,031	28,429,522
1861–65	5,937,245	35,328,562
1871–75	5,579,410	63,187,718
1881–85	4,939,831	85,569,149
1891–95	7,895,166	160,862,156

The first of these quinquenniums was the period of the rush to California and Australia, and of the workings of rich placer diggings in both of these great gold fields. As compared with the average annual production of the preceding ten years, there was an increase of 4,640,180

ounces fine gold. But as the placer diggings were gradually worked out, the annual output slowly decreased until in 1881–85 it fell below the average of the previous thirty years by 1,457,200 ounces. The lowest point was touched in 1883, when the production was only 4,614,588 ounces. Soon after that came the extension of the gold fields of Australia and the enlarging of operations in the United States, followed in 1890 by the development of the Randt; and the annual average of the last quinquennium showed an increase of 1,498,135 ounces over the first. For the year 1896 the production was 10,167,817 fine ounces, being an excess over the annual average of 1851–55 of 3,770,786 ounces, and over that of the decade 1841–50 of 8,410,966 ounces.

The increase in average annual production of silver from 1851–55 to 1861–65 is shown to be 6,899,040 ounces. Then ensued the workings of the great bonanza mines on the Pacific slope, accompanied a little later by extended operations in Mexico and developments in Australia — and so the annual average of 1851–55 was exceeded in 1871–75 by 34,758,196 ounces, in 1881–85 by 57,189,627 ounces, and in 1891–95 by 132,432,634 ounces. For the year 1896 the excess over the average of 1851–55 was 157,612,666 ounces, and every year but one of the last twenty years shows a steady increase of production, rising from 62,679,916 ounces in 1877 to 186,042,188 ounces in 1896.

The relations of the two metals by weight and commercial value are shown in the next table for the periods under comparison, gold being taken as the unit:

YEARS.	WEIGHT.	VALUE.
1851–55	1: 4.44	1:15.41
1861–65	1: 5.92	1:15.41
1871–75	1:11.33	1:15.97
1881–85	1:17.32	1:18.59
1891–95	1:20.37	1:27.27

The third period, when there was a large increase in the ratio of production, shows also an increase in the ratio of value; but as the demonetization of silver occurréd in 1873, it may be said that this was the cause of the fall in the price of silver. The downward movement, however, began as early as 1866, or almost simultaneously with the relative increase in the production of silver, and it became accentuated as the disproportion in the production of the two metals went on. In 1894 the price of silver reached its lowest point, the ratio in that year being as 1: 32.60, while the ratio of production was as 1 to 19. In the next year the ratio of production was 1: 18.71 and the ratio of value was 1: 31.65; and in 1896 the production was 1: 18.30 and value 1:30.80.

Apparently these figures show, as conclusively as statistics can prove anything, that, beyond some indefinite ratio in the production of the two metals, the price of silver fell as the relative production of it increased, and that as the relative production of gold increased the price of silver rose, or, perhaps more accurately stated, the price of gold fell.

Under these circumstances, and with freedom of production unhindered in any way, could the maintenance of a double standard and free coinage have led to any other result? Is it in the power of governments, by any law or regulation short of the autocratic limitation of the production of gold and silver throughout the world, to fix and maintain a ratio in the value of the metals in currency or commerce that shall not change or vary with a disturbance of the production ratio? Is there any law of the uncanny that applies to metal as coin, and does not apply to it as an article of use in the arts?

Suppose we take other metals besides gold and silver and try to ascertain how a double or quadruple standard would apply in their case. We may take nickel and aluminium, both of which possess qualities suitable for coinage purposes but around which there has gathered no glamour of prescription.

Nickel has been used as a coin metal in the United States and Mexico, as well as in several countries of continental Europe. Twenty years ago, when the only mine in America that yielded nickel was the little mine at Lancaster Gap in Pennsylvania, the average price of the refined metal was $3 per pound avoirdupois, being $2.49 per pound troy or 20¾ cents per ounce. At the gold standard this would be represented by a ratio of 1 to 100. Since then the extensive nickel deposits of New Caledonia and Ontario have been discovered and opened up, and the processes of treating the ore and refining the metal have been greatly improved; and although nickel continues to be coined into money its commercial price in a free market has fallen to 28 cents per pound avoirdupois, being 23 cents per pound troy, or less than two cents per ounce, which at the gold standard would be represented by a ratio of 1 to 1000. At the present rate of working, the mines of Ontario could produce 2,500 tons of refined nickel a year and that output might readily be increased five-fold or ten-fold. Is there any power in governments or conventions which could support the free coinage of nickel at a standard of 1 to 100 when the commercial value of the metal has been divided by 10?

In 1854, when the first sample of refined aluminium was obtained from its ore, an ounce cost almost as much as an ounce of gold. Three years later, by an improved process of treatment, it was produced at $60 per kilo, which is $27.27 per pound avoirdupois, or $1.87 per ounce troy. Compared with gold as the standard, this would be represented by a ratio of 1 to 11. Better and cheaper methods were adopted in the course of a few years, and in 1893 there was manufactured in the United States 295,000 lbs. avoirdupois of the metal, valued at $191,750, or nearly four and one-half cents per ounce troy. In 1896, with larger facilities of production, the output was increased to 1,300,000 lbs. avoirdupois, valued at $540,000, or very nearly three cents per pound troy. At this value aluminium would compare with the gold standard as 1 to 689 instead of as 1 to 11, the ratio of forty years ago.

Now with the great increase that has taken place in the production of nickel and aluminium, and the invention of methods for cheaper separa

tion of the metals from their ores, is it conceivable that the values of twenty or forty years ago could be kept down to the present time by the simple scheme of selecting these metals along with gold and silver for coinage uses and fixing a conventional standard? Or would not the same thing happen in their case as in the case of silver when the dislocation of ratios of production took place?

The great invention of Sir Henry Bessemer made possible the girding of this continent with railways. Since the first shops were opened in Sheffield thirty-five years ago to apply Bessemer's process, the price of steel rails has fallen from $120 to $20 and less per ton, freight charges have fallen proportionately, and areas of land equal in extent to whole empires of the old world have been brought under cultivation in the new. One of the first fruits of this enlargement of farmland on the prairies of the western states and of northwestern Canada was the slump in the price of wheat, for the increase in the quantity grown was much greater than the increase in the number of consumers. But just as the equilibrium is about being restored again, another enlargement of the area of wheatland looms up with the prospect of an early completion of the railway across Siberia. A few days ago it was announced that the government at Washington proposed to send a commission into northern Asia to ascertain its fitness for the growth of wheat, no doubt with the praiseworthy object of preparing the farmers of the United States for the situation this possible rival region may create.

It is well to be forewarned, but new fields for the cultivation of wheat may not be the only disturbing element that confronts us. The large increase in the world's production of gold, which had its beginning with this decade, has already begun to tell upon the relative values of gold and silver, and no one can say what the end will be. Within two years, during which the production of silver was increased by 19,440,193 ounces, the ratio of gold and silver production fell from 1 to 19 to 1 to 18.30, and the ratio of value from 1 to 32.60 to 1 to 30.80. Other gold fields are opening, some in northern Ontario and some in the Yukon territory of northwestern Canada. Fabulous stories of the richness of the Klondike valley are told; single pannings are reported to yield nuggets of metal ranging in value from $500 to $1,000; and other valleys in the territory hundreds of miles in extent are supposed to hold as rich placers as the Klondike. These diggings promise to add very largely to the world's production of gold, and when the quartz veins in the mountains are discovered out of which the nuggets have been washed down by the streams it is not improbable that the records of the mines of California, Australia and Witwatersrandt may cease to excite our wonder or stir our avarice. An addition of 5,000,000 ounces to the world's yearly production of gold would inevitably tend to bring down the ratio toward the old standard around which gold and silver played for nearly two centuries preceding the dislocation whose origin we trace to the development of the silver mines of America and Australia within the last thirty years.

The gold wealth of Yukon would appear to be as worthy of investiga-

tion by governments engaged in studying the problem of a gold standard or the free coinage of silver, as is the probability of the Siberian steppes becoming troublesome rivals of the American prairies in growing wheat supplies for the world's markets.

Joint sessions of the Section with the Society for the Promotion of Agricultural Science were held on August 12 and 13.

EXECUTIVE PROCEEDINGS.

REPORT OF THE GENERAL SECRETARY.

GENERAL SESSION, MONDAY MORNING, AUGUST 9.

THE first General Session of the Forty-sixth Meeting of the American Association for the Advancement of Science was called to order at 10.45 A. M., Monday, August 9, 1897, in the Auditorium of the Central High School of Detroit, Professor THEODORE GILL, the senior Vice President of the Buffalo Meeting presiding on account of the decease of the retiring President EDWARD D. COPE. Prayer was offered by the Rev. Father VAN ANTWERP; after which a musical selection was rendered by Mr. MARSHALL PEASE.

Professor GILL then introduced Professor W J MCGEE, the senior Vice President of the meeting, as the acting President in the absence of Dr. WOLCOTT GIBBS, the President elect of the Detroit Meeting. Professor MCGEE read a letter from Dr. GIBBS expressing his regret at being unable to be present on account of the condition of his health.

Professor MCGEE then introduced His Honor MAYOR MAYBURY, who welcomed the Association in part as follows:

It is with a very pleasurable feeling that I welcome you, ladies and gentlemen, to this hospitable city. During the months of this summer, now passing away, it has been my pleasure as mayor of the city to welcome all sorts and conditions of men. We have welcomed those who came for pleasure as well as those who came for business; those who were of a religious turn of mind as well as those who were seeking for good fellowship only. Upon all these people we have endeavored to leave the impression that we were hospitable, and that as a people we were as care-free as any, and yet calm and serious minded.

We may claim to be something of a scientific city, founded upon this fact: there was formed here years ago, by the late Bela Hubbard, a man of God-like character, a society which was in fact the beginning of your great organization. We have established the place of your meeting in a building devoted to science and to education. If environment serve to influence the character of your work, we may well hope that your best work may be done here within these walls.

Our hospitality is intended to be unconditional and unconfined, but I would like to ask of you a personal favor. I wish you would add to your

program a discussion of a very important municipal subject, which might be paraphrased as follows: 'How can the mayor of a goodly city coerce a wicked and recalcitrant council?' or, if you think it best, we might have a scientific discussion and decision of the opposite question, 'How can a wise and patriotic council lead into the ways of righteousness a weak and headstrong mayor?' If you will solve this question scientifically for us we shall be most amply repaid for all that we can possibly do for you.

There is to my mind no better illustration of the progress of the times towards right and justice than the scene of the happy coördination now going on between religion and science. A true science fits and confirms a true religion, and no religion worthy of the name is to-day unwilling to have its merits considered in the light of the most advanced scientific discovery. Happily it may be said that so confirming have the truths of one been of the truths of the other that they are recognized as handmaidens.

We hope that your meeting here will be pleasant, as we have endeavored to make the environment all that could be desired. You are visiting a city that has passed through more than one dispensation, beginning with the early French explorer, then living under the Cross of King George, and now and forever under the Stars and Stripes. We trust you will go about this Zion of ours, having enjoyed the beauty of our palace and cottage homes, so that you may be able to tell those who come after. Again I bid you welcome.

Professor McGee next introduced Honorable Thomas W. Palmer who spoke as follows: Amid the general scramble for money, place and power which characterizes the present age it is gratifying to know that there is an increasing number of men and women who, ignoring the common objects of ambition, have devoted themselves to, and are diligent in the unselfish pursuit of truth. They are those who believe that not all of truth is contained in papyri and palimpsest, or within the covers of sacred books, but that the Almighty is constantly revealing Himself in the lamina of the rocks, in the sublime march of the glaciers, in the lightning, the wind, the storm, in the firmament; in the verdure of the field, the foliage of the forest, and in all insect and animal life.

Although the lips of prophecy have long been sealed and the inspired pen no longer transcribes its messages, these men and women find, in the phenomena of nature, history related with accuracy and predictions certain to be verified. Formerly it was considered that there was a conflict between science and religion. Let us hope that that day has long been passed and that the more enlightened public recognizes that religion has to do with the spiritual nature of man and science with physical phenomena. And what is science? I have asked the question of many, and some have said with hesitancy, why, science is science, and until I had given the subject careful thought I would have made the same answer. Herbert Spencer defines it as the extension of our perceptions by means of reasoning; but he admits that that is not a full definition.

For the benefit of myself and others who are seeking for a popular definition I would offer the following in deep humility, for I know nothing about science, hoping that it may be assailed and discussed until a proper definition is arrived at. I would define science as "the classification of phenomena to the end that principles may be established and declared, from which may be deduced rules of action which shall be applicable in particular cases."

Where did science first originate? Back of the dawn of history, when primeval man, emerging from his shelter of the cave, before he had even built him a hut, commenced to compel the forces of nature to his use. It must have made many strides before Assyria or Chaldea arose above the horizon, for we know that the Chaldeans had advanced ideas of astronomy, and Job himself, although a poet, must have associated with scientific men or been in the vicinity of a scientific society, for he speaks of Orion among the constellations and Arcturus and the Pleiades and, undoubtedly if Arabic had been invented at that time, he would have spoken of Aldebaran. He must have heard of natural history judging from the use that he makes of such knowledge in his writings.

Again Egypt must have had a knowledge of mechanics and engineering before she erected "those temples, piles and monuments stupendous, of which the very ruins are tremendous." And if we read aright she must have had knowledge of acoustics if the story of Memnon discoursing music to the rising sun be true.

How did science originate? By extended observations, experience and comparison. The first savage who played in the water with his reed, as a baby does with a straw blowing and sucking it, gave the first illustration of hydraulics. The little savage playing in the woods at the ancient game of see-saw would find that the heaviest boy had to sit nearest the center, and this would give the first suggestion of the lever which, repeated and experimented upon and modified, led to the lever with which Archimedes proposed to raise the world if he only had a fulcrum. The first man who gave a formula for scientific methods, as far as I know, was Aristotle when he declared that "all reasoning must be based upon facts."

Then came Archimedes with his invention of the screw, and all of those devices with which history delighted our boyish eyes in reciting how he defended Syracuse. Then came the Ptolemies, who were philosophers as well as kings, and who described a degree of longitude upon the sand of the Mediterranean. They made Alexandria a school of scientific men. In Alexandria they had libraries, zoölogical and botanical gardens, dissecting and astronomical schools. I forgot to state that, nearly two thousand years before Magellan circumnavigated the globe, Eratosthenes had declared the earth to be globular and measured its circumference by astronomy.

In the dissoluteness of the first years of the Roman Empire, Science, slighted and discouraged, made no progress, and during the dark ages, scorned and persecuted, she fled from christendom to the courts of Cordova and Bagdad. Her rehabilitation commenced with the downfall of

the Greek empire when bright men fled from Constantinople to the west and gave an impulse which the world feels to-day. Then came Columbus, Copernicus, Galileo, Bruno, Galvani, Volta, Watt, Newton, La Place, La Voisier, Priestly, Stephenson, Darwin, Huxley, Tyndall, Lubbock, and in our own country, Franklin, Morse and Fulton. These great men have enthroned science as a goddess demanding no homage, but which all thinking men look up to with reverence and respect. She has been the handmaid of religion in elevating humanity, and with each successive step new vistas open revealing greater wonders beyond. She has dragged gold from the mine and filled the coffers of the rich, she has furnished fine linen within which they may sleep. She has given them purple for vesture, asking nothing in return. Like the blessings of Heaven her benefits fall upon the just and the unjust.

She has lifted the poor to comfort and taught the rules of correct living. She has manacled the pestilence which stalketh at noonday and strangled the Afrite of the sewers. She has declared the laws of sanitation so that he who runs may read.

What her future will be no man can foretell, but we know that it will accumulate untold blessings upon the race.

To you, gentlemen of the American Association for the Advancement of Science, we may say that we regard your meeting here as a red letter day for our city. Where you hold your sessions, a century ago the Indian wielded his tomahawk and the wild fox dug his hole unscared. May this convocation be productive of good results as I know it will, for the establishment of truth cannot but be beneficent to humanity.

In responding to the addresses of welcome, Acting-President McGee noted the advance of science in this country since the institution of the Association. The organization has been in existence nearly half a century; it was created to meet a wide-felt need on the part of American citizens for definite information concerning the phenomena of nature; and during each year of its existence it has diffused exact knowledge by means of public addresses and technical papers as well as through the published proceedings. During the half century of its existence, scientific progress has been incomparably more rapid than ever before, and the methods and purposes of research, as well as the mental characteristics of the investigator, have become known among the people so generally as to affect thought in every community and in nearly every family. A half-century ago the very name of science was unknown to most of our people; to-day science is inculcated, not only in the universities but in the colleges and even in the primary schools; to-day the literature of science fills great libraries; to-day every magazine designed for cultured readers contains half as much of science as of pure literature; while the daily metropolitan press and even the weekly village newspapers regularly contain columns of matter designed to be scientific in character. When the Association was created, science was regarded by its few votaries, and by the limited circle of laymen who knew of it, as something apart from every-day interests; now the

fruits of science are within the reach of all men, and there is no important industry in this great industrial country that is not shaped by scientific research; now the phenomena of nature are known, and the knowledge is a power daily utilized in controlling our magnificent natural resources. The growth of America during the last half century has been beyond parallel in the history of nations; and this growth may be, indeed must be, ascribed to a single great cause, *i. e.*, the conquest of natural forces, the subjugation of natural conditions, the utilization of natural resources, through scientific research. So the history of the half century has been one of unprecedented scientific progress, and, at the same time, one of unequaled material progress through the diffusion and utilization of science for the promotion of human welfare; and, while many instrumentalities have contributed indirectly toward making this history, there is but one which has contributed directly, intentionally, and constantly, and that is the Association for the Advancement of Science to which the far-sighted citizens of Detroit have extended a cordial invitation and a most hospitable reception.

The members of the Association may well rejoice in the evidence of the success of their efforts visible on every hand; their coming was hastened and made easy by mechanical devices, many of which, invented since the Association was created, were first made known in its meetings; they are received in homes and hostelries made pleasant and healthful through sanitary and hygienic devices whose germs they nursed; they are greeted by a statesman whose well-weighed words demonstrate their success in diffusing scientific method in the inner circles of statecraft, and by a municipal administrator whose justly-appreciated strength lies in his command of ideas based on scientific inquiry. This officer, withal, is modest, as befits the student, and begs for guidance at the hands of the wise men within his gates; but they are no less distrustful of their own sagacity, and, after viewing the magnificent streets of one of the world's fairest cities, the superb public buildings, the air of law and order pervading Detroit, and the innumerable other indications of successful municipal control, can only assure him that they, not he, must take the place of the learner. It must be especially gratifying, too, to every member of the Association, interested as they are in education in its deepest and broadest sense to note the evidence of wise and liberal educational policy displayed in Detroit. The Central High School building assigned to the Association for the use of this meeting is the fittest and most comfortable meeting-place of recent years; and at the same time it is a model structure for school purposes, apparently the finest and best in all our land of public schools.

Hospitality is an inspiration, intelligence challenges intelligence, and culture breeds culture; so the hospitality extended to the Association and the intelligence and culture of the hosts and hostesses of Detroit presage a successful meeting.

Professor McGEE's remarks were followed by musical selections rendered by Miss MARY L. DENISON and Mr. MARSHALL PEASE.

The following announcements were made by the GENERAL SECRETARY:

1. The COUNCIL recommends that the daily sessions be held from 10 to

12, and from 2 to 5 P. M. on Monday, Tuesday, Wednesday, Thursday and Friday.

The recommendation was unanimously adopted.

2. The COUNCIL nominates L. O. HOWARD of Washington as Vice President of Section F, to fill the vacancy caused by the death of Dr. G. BROWN GOODE.

Dr. HOWARD was unanimously elected Vice President.

3. In case any section chooses to appoint a Press Secretary he shall be elected on the nomination of the Secretary of the Section.

A letter from Mr. HENRY M. UTLEY, Librarian of the Public Library of Detroit, was read, extending to the Association the privileges of that library.

After announcements by Mr. JOHN A. RUSSELL, the LOCAL SECRETARY, the session adjourned.

EVENING SESSION, MONDAY, AUGUST 9.

The Association assembled at 8.30 P. M., ACTING PRESIDENT McGEE in the chair. A memorial address on Professor EDWARD D. COPE, the dedeceased president of the Buffalo meeting, was delivered by Professor THEODORE GILL of Washington.

After the address a reception was given to the Association in the High School building by the members of the LOCAL COMMITTEE.

GENERAL SESSION, TUESDAY MORNING, AUGUST 10.

The General Session was called to order at 10 A. M., ACTING PRESIDENT McGEE presiding. The GENERAL SECRETARY announced the following action by the COUNCIL:

1. E. W. CLAYPOLE was nominated as Chairman of Section E, to fill the vacancy caused by the absence of Professor WHITE. Professor CLAYPOLE was unanimously elected Vice President.

2. The privileges of the Association were granted without fee to visitors who are members of foreign societies of high standing.

3. When a past president or vice president visits a Section the presiding officer is to invite him to a seat on the platform.

An invitation was read from HUGHES and SARGEANT, photographers, requesting the Association to assemble at 12.30 to be photographed.

An invitation was read from PARKE, DAVIS & Co. for Section C and other members of the Association to visit their chemical works.

A communication was received from R. C. HILL, Secretary of the Pan-American Exposition at Niagara Falls in 1899, urging that the meeting of the Association for '99 be held at that place.

An invitation was read from Mr. JOSEPH BERRY of Grosse Pointe to a reception for the members of Section G, officers of the Association, and the foreign guests.

A report was received from the Committee on *Science Teaching*. No meeting had been held and the Committee was continued by the COUNCIL.

The Committee on *The Library of the Association*, which was deposited with the University of Cincinnati in 1896, reports that it is properly cared for.

The Committee on *Indexing Chemical Literature* reported to the Association.

On account of the illness of Dr. BRINTON, the Committee *for the study of the White Race in America* was continued by the COUNCIL.

Section H was granted permission by the COUNCIL to hold a winter meeting.

The Session then adjourned.

GENERAL SESSION, WEDNESDAY MORNING, AUGUST 11.

The Association was called to order at 10 A. M. by ACTING PRESIDENT McGEE.

The appointment by the COUNCIL of the following Committee to secure uniform nomenclature in scientific work was announced: Messrs. MORSE, PRESCOTT, WOODWARD, HOWARD, and COLBURN as chairman.

The GENERAL SECRETARY announced that the COUNCIL had elected Mr. JOHN A. RUSSELL, the Local Secretary, a member without fee.

After notices by the LOCAL SECRETARY the Association adjourned.

GENERAL SESSION, THURSDAY MORNING, AUGUST 12.

The General Session was called to order at 10.10 A. M. by ACTING PRESIDENT McGEE.

The Council elected as Fellows the following Members of the Association:—

Aldrich, Professor William Sleeper, West Virginia Univ., Morgantown, W. Va. (43). D

Barrows, Wilber B., Agricultural College, Ingham Co., Mich. (40) F

Bascom, Miss Florence, Bryn Mawr Coll., Bryn Mawr., Pa. (42). E

Bigelow, Willard Dell, Chem. Div., Dept. of Agric., Washington, D. C. (44). C

Bowditch, Charles P., 28 State St., Boston, Mass. (43). H

Bull, Prof. Storm, Madison, Wis. (44). D

Chester, Commander Colby M., U. S. N., U. S. Naval Academy, Annapolis, Md. (28). D

Clarke, John Mason, Ass't State Geol. and Paleontologist of N. Y., State Hall, Albany, N. Y. (45). E

Cowles, Alfred H., 656 Prospect St., Cleveland, Ohio (37). B C

Daniells, Prof. William W., Univ. of Wis., Madison, Wis. (42) C

Davis, Bradley Moore, Dep't of Botany, Univ. of Chicago, Chicago, Ill. (45). G

Golden, Miss Katherine E., Lafayette, Ind. (42). G

Greene, Prof. Edward Lee, Univ. of Cal., Berkeley, Cal. (42). G

Gunckel, Lewis W., 435 W. Second St., Dayton, Ohio (41). B

Guthe, Karl E., Ph.D., Instructor in Physics, University of Michigan, 36 Kingsley St., Ann Arbor, Mich. (45). **B D**
Hagar, Stanbury, Brooklyn, N. Y. (43). **H**
Hill, John Edward, Brown Univ., Providence, R. I. (44). **D**
Horsford, Miss Cornelia, 27 Craigie St., Cambridge, Mass. (43). **H**
Hrdlecka, Ales, M.D., 823 Park Ave., New York, N. Y. (46). **H**
Humphrey, James Ellis, Johns Hopkins Univ., Baltimore, Md. (44). **G**
Keep, Wm. J., Supt. Mich. Stove Co., Detroit, Mich. (37). **D**
Kendrick, Prof. Arthur, Rose Polytechnic Inst., Terre Haute, Ind. (45). **B**
Kenyon, Frederick C., Washington, D. C. (46). **F**
Lambert, Preston A., 215 S. Center St., Bethlehem, Pa. (41). **A**
Loubat, le Duc de, 47 rue Dumont-d'Urville, Paris, France (46). **H**
Ludlow, Lt. Col. Wm., Corps of Eng., U. S. A., Army Building, New York, N. Y. (33). **D B**
Mac Dougal, Daniel T., Univ. of Minnesota, Minneapolis, Minn. (44). **G**
Moore, Clarence B., 1321 Locust St., Philadelphia, Pa. (44). **H**
Moore, Prof. Willis L., Chief of Weather Bureau, Dept. of Agric., Washington, D. C. (44). **B**
Moulton, Prof. Charles W., Poughkeepsie, N. Y. (44). **C**
Noyes, Prof. Arthur A., Mass. Inst. Technology, Boston, Mass. (45). **C**
Orleman, Miss Daisy M., M.D., Peekskill Military Academy, Peekskill, N. Y. (40). **F**
Parks, C. Wellman, U. S. Patent Office, Washington, D. C. (42). **D**
Peale, Albert C., M.D., U. S. Geol. Survey, Washington, D. C. (36). **E**
Peirce, George James, Botanical Dept., The University of Indiana, Bloomington, Ind. (44). **G**
Ries, Heinrich, Ph.B., Columbia College, New York, N. Y. (41). **E**
Ripley, William Z., Ph.D., Newton, Mass. (44). **H I**
Russell, Frank, Instructor in Anthropology, Harv. Univ., Peabody Museum, Cambridge, Mass. (45). **H**
Sharp, Dr. Clayton H., Ithaca, N. Y. (45). **B**
Snyder, Prof. Harry, St. Anthony Park, Minn. (44). **C**
Speyers, Clarence L., Rutgers College, New Brunswick, N. J. (36). **C**
Spinney, L. B., Ames, Iowa (42). **B**
Taylor, F. B., Box 2019, Fort Wayne, Ind. (39). **E**
Thornburg, Charles L., Prof. Math. and Astron., Lehigh Univ., South Bethlehem, Pa. (44). **A**
Wagner, Frank C., care Wm. Wagner, Ann Arbor, Mich. (34). **D**
Washington, Dr. Henry S., Locust, N. J. (44). **E**
Whitman, Prof. Charles O., Chicago, Ill. (43). **F**
Willoughby, Charles C., Peabody Museum, Cambridge, Mass. (45). **H**
Witmer, Lighton, University of Pennsylvania, Philadelphia, Pa. (46). **H**
Woll, Fritz Wilhelm, Madison, Wis. (42). **C**
Woods, Albert F., U. S. Dept. Agric , Washington, D. C. (43). **G**
Worcester, Dean C., Ann Arbor, Mich. (46) **F H**

It was announced that the COUNCIL recommends to the favorable consideration of the Association the movement to raise funds for a statue GALILEO FERRARIS.

Through the COUNCIL a report was received from the Committee on the *Unification of Scientific Nomenclature*. The report was accepted by the COUNCIL and ordered printed by the PERMANENT SECRETARY.

It was announced that the COUNCIL had repealed the action taken at the Buffalo Meeting, abridging the amount of matter to be printed in the annual volume. On motion of Mr. HALE a resolution was passed approving the action of the COUNCIL in returning to the former method of publication.

From the WATER COMMISSIONERS of Detroit an invitation was received for the Association to visit the water works.

After notices by the LOCAL SECRETARY the session adjourned.

GENERAL SESSION, FRIDAY MORNING, AUGUST 13.

The General Session was called to order at 10.40 A. M. by ACTING PRESIDENT McGEE.

The following announcements from the COUNCIL were made by the GENERAL SECRETARY:

1. The Secretary of Section C presented to the COUNCIL the report of Mr. ISAAC OTT on his investigation of the physiological effect of Glucinum chloride on cold and warm blooded animals.

2. The COUNCIL allows a grant of $100 to the Marine Biological Laboratory at Woods Hole for 1898, the Committee on the table at this laboratory to consist, for 1898, of the incoming and outgoing vice-presidents of Sections F and G, together with the director of the laboratory.

This grant the treasurer was authorized to make from the research fund.

3. Miss LUELLA AGNES OWEN made a gift to the Association of $10. Professor F. C. ROBINSON made a gift of $17. These gifts were gratefully accepted by the COUNCIL and the TREASURER was instructed to place the sums to the credit of the permanent funds of the Association.

4. The COUNCIL appointed the following as a Committee on *Extending the Influence of the Association into the Secondary Schools:* E. S. MORSE, W. ORR, jr., T. C. CHAMBERLIN.

A statement was made by the GENERAL SECRETARY regarding the place of meeting of the Association in 1898, the jubilee year of the Association. Invitations were received from Cincinnati, Omaha, Niagara Falls and the following urgent invitations were received also to meet in Boston.

Boston Society of Natural History,
Boston, Massachusetts, June 9, 1897.

PROF. F. W. PUTNAM,
SEC'Y AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.
DEAR SIR:—

At a special meeting of the Council of the Boston Society of Natural History held to-day it was voted that the Council, in behalf and in the name of the Society, invite

the American Association for the Advancement of Science to hold the Fiftieth Anniversary of the Association in 1898, in the City of Boston.

The Society can assure the members of the Association a cordial welcome on the part of the people of Boston and Cambridge.

Yours truly,

SAM'L HENSHAW,
Sec'y.

Commonwealth of Massachusetts, Executive Department,
Boston, July 2, 1897.

MESSRS. F. W. PUTNAM, *Chairman*; CHAS. S. MINOT, *Pres.*; SAM'L HENSHAW, *Sec'y*;
COMMITTEE OF THE COUNCIL OF THE BOSTON SOCIETY OF NATURAL HISTORY.

GENTLEMEN:—

I have heard with much interest that it is the purpose of the Boston Society of Natural History to extend an invitation to the American Association for the Advancement of Science to hold its fiftieth anniversary in the city of Boston in August, 1898. I earnestly trust that this purpose may be accomplished.

It would seem to be especially proper that the Association should complete its half century on the soil where it was born, and we of Massachusetts believe that nowhere in the country would the Association receive a more cordial reception or be able to hold a meeting that would be more likely to prove agreeable and interesting to those who participate in it. Ever hospitable to all movements for the advancement of education and science the Commonwealth will extend a warm welcome to the members of this distinguished Association.

Very truly yours,

ROGER WOLCOTT.

City of Boston, Office of the Mayor,
City Hall, June 24, 1897.

F. W. PUTNAM, ESQ.,
CHAIRMAN OF COMMITTEE OF COUNCIL, OF BOSTON SOCIETY OF NATURAL HISTORY, CARE OF BOSTON SOCIETY OF NATURAL HISTORY, BOSTON, MASS.

DEAR SIR:—

I beg to acknowledge the receipt of the communication dated June 18th, signed by yourself and others on behalf of the Boston Society of Natural History. I am glad to learn that it is proposed to hold the fiftieth anniversary of the American Association for the Advancement of Science in Boston next year. On behalf of the city, I take pleasure in extending to the Association a cordial invitation to come to Boston for its meeting of next year, and if the Association decides to meet here the city authorities will certainly do anything in their power in the way of assisting in the arrangements. I am sure that our citizens would esteem it an honor to have the Association select Boston as the place in which to celebrate its fiftieth anniversary.

Yours respectfully,

JOSIAH QUINCY,
Mayor.

Massachusetts Institute of Technology, Boston, July 30, 1897.

PROF. FREDERICK W. PUTNAM,
SECRETARY AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE:

DEAR SIR:—

The Corporation and Faculty of the Massachusetts Institute of Technology have learned with great interest that other institutions, in or near Boston, will extend to the American Association for the Advancement of Science an invitation to hold its Jubilee meeting of 1398 in this city.

We join most cordially in the invitation, recalling with gratification the intimate connection of our first President, William Barton Rogers, with the early years of the Association, and beg to assure you that we shall use every effort in coöperation with other institutions and societies to make the meeting of 1898 a success worthy of the Association and of the city.

Appreciating your courtesy in transmitting this message to the Association, we are,

Respectfully yours,

J. M. CRAFTS,
Chairman of Faculty.
HARRY W. TYLER,
Secretary.

Harvard University, Cambridge, June 22, 1897.

The President and Fellows of Harvard College desire to unite with other Massachusetts institutions of learning in inviting the American Association for the Advancement of Science to hold its fiftieth anniversary in Boston, in August, 1898; and they have the honor to invite the Association to visit the grounds, buildings and collections of Harvard University on one of the days of the meeting, and to take luncheon with them in Memorial Hall.

For the President and Fellows of Harvard College,

CHARLES W. ELIOT,
President.

Boston University, President's Office, June 23, 1897.

MESSRS. F. W. PUTNAM, CHARLES S. MINOT AND SAMUEL HENSHAW, *Committee.*

GENTLEMEN :—

Your welcome communication of the 9th inst. reached me too late to be laid before our University Council. Vacation had already begun and every member but one was absent in Europe or elsewhere. Not until to-day have I had opportunity to consult even with Dean Huntington of our College of Liberal Arts who has now returned for a few days. Personally and—so far as is proper—officially, I desire to join in the proposed invitation of the Boston Society of Natural History to the American Association for the Advancement of Science to hold its fiftieth anniversary in our city. I regret that I have not the formal and express authorization of the Council, but I have no doubt my colleagues would desire me to write with equal heartiness in their name.

Wishing you success in this and in all other good undertakings, I remain, as ever, with high respect,

Yours faithfully,

W. F. WARREN,
President Boston University.

Massachusetts Horticultural Society, Boston, July 3, 1897.

PROFS. F. W. PUTNAM, *Chairman;* CHAS. S. MINOT, *Pres.;* SAM'L HENSHAW, *Sec'y;*
COMMITTEE OF THE COUNCIL BOSTON SOCIETY OF NATURAL HISTORY.

GENTLEMEN :—

At a meeting of this Society held this day, upon advice of the Executive Committee after hearing yours of June 9th, and after the reading of your letter, it was voted, as in the second vote herewith enclosed.

I may say that yours of June 9th was brought before the first meetings of Executive Committee, and of this Society, since its receipt.

At present President of the American Forestry Association, which has usually joined with the American Association for the Advancement of Science, I trust they will also join in the meeting and be included in the special rates.

Respectfully yours,

FRANCIS H. APPLETON,
President.

Voted: — That the Executive Committee advise that the Massachusetts Horticultural Society unite with the Boston Society of Natural History in inviting the American Association for the Advancement of Science to hold its meeting (fiftieth anniversary) in 1898 in the city of Boston.

Voted: — That the Massachusetts Horticultural Society unite with the Boston Society of Natural History in inviting the American Association for the Advancement of Science to hold its meeting (fiftieth anniversary) in 1898 in the city of Boston.

Appalachian Mountain Club, Boston, Mass., July 28, 1897.

PROF. F. W. PUTNAM.

DEAR SIR: —

It gives me pleasure to inform you that the "Appalachian Mountain Club" joins other organizations in Boston in extending an invitation to the American Association for the Advancement of Science to hold its Jubilee meeting, in 1898, in Boston.

Sincerely yours,

ROSEWELL B. LAWRENCE,

Recording Secretary.

American Academy of Arts and Sciences,
Boston, June 15, 1897.

TO THE SECRETARY OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

SIR:—

I am directed by the Executive officers of the American Academy of Arts and Sciences to extend to the American Association for the Advancement of Science a cordial invitation to hold their fiftieth anniversary, in 1898, in the city of Boston. The Academy itself, which unfortunately holds no stated meeting until the autumn, will be asked on the first opportunity to ratify this action of its officers and will doubtless do so with unanimity.

No more fitting place than the city in which it was organized can be found for the Jubilee meeting of the Association, and the Academy will take every possible measure to make the meeting rival in interest that held here in 1880.

Respectfully yours,

SAMUEL H. SCUDDER,

Corresponding Secretary.

Tufts College, Mass., June 19, 1897.

TO THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

GENTLEMEN:—

In behalf of the Trustees and Faculty of Tufts College, I hereby extend an invitation to your Society to hold its meeting commemorating the fiftieth anniversary of its founding in the city of Boston.

Tufts College will be glad to do what it can in coöperation with other institutions of learning to make the meeting pleasant and profitable.

Very respectfully,

ELMER HEWITT CAPEN,

President.

The following letter was received after the adjournment of the meeting.

Rooms of the Essex Institute, Salem, Mass., Aug. 20, 1897.

TO THE PRESIDENT OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

DEAR SIR:—

By vote of the Board of Directors of the Essex Institute, I am instructed to extend to the American Association for the Advancement of Science a cordial invitation to devote one day, during the next year's annual meeting, to a visit to Salem.

In transmitting this vote, I need not add that I heartily concur in the action taken,

And am with great respect,

Yours truly,

ROBERT S. RANTOUL,

President.

The resignation was announced of PERMANENT SECRETARY PUTNAM, to take place at the beginning of the next meeting, and appropriate remarks regarding Mr. PUTNAM'S services to the Association were made by ACTING PRESIDENT MCGEE.

Certain amendments to the Constitution proposed at the Buffalo meeting were brought up and after discussion by Messrs. HALE, FAIRCHILD, WOODWARD, MORSE and others, were passed, having more than the necessary three-fourths vote of those present.

These amendments were:

1. In Article 9, in place of 1st and 2nd lines, strike out *in General Session* and insert *by the Nominating Committee* so that the first clause of ART. 9 shall read, *The officers of the Association shall be elected by ballot by the Nominating Committee.*

2. In ART. 17, the second sentence shall read:

Vacancies in the offices of Vice President, Permanent Secretary, General Secretary, Secretary of the Council, and Treasurer, shall be filled by the Council by ballot.

3. In Article 19 the second sentence shall read: *It shall be the duty of this Committee to meet at the call of the President and elect the general officers for the following meeting of the Association.*

4. In Article 19, the third sentence shall read: *It shall be the duty of this Committee to fix the time and place for the next meeting.*

5. In Article 20, the first sentence was amended to read:

The Association shall hold a public meeting annually, for one week or longer, at such time and place as may be determined by vote of the Nominating Committee, etc.

6. In Article 37, the word *admission* was stricken out.

The question being raised, Acting-President MCGEE ruled that the amendments to the constitution took effect immediately on adoption. He also expressed the opinion that the selection of officers for the ensuing year by the Nominating Committee was, under the amended constitution, tantamount to election, and invited contrary opinion; no objection was made; he then ruled that the officers chosen by the Nominating Committee were thereby elected, again inviting contrary expression. No objection being made, the officers chosen by the Nominating Committee were declared elected.

[For list of officers elected for the next meeting see page x of this volume.]

In accordance with the action of the Nominating Committee, Boston was announced as the place of meeting for 1898.

It was announced that the COUNCIL approved the action of a Committee of Section A recommending that the Association should take no action regarding the change of the time of beginning the Astronomical day.

The PERMANENT SECRETARY read the list of members deceased since the last meeting. [See page LXXIX of this volume.]

The LOCAL SECRETARY made several announcements after which the session adjourned.

FINAL GENERAL SESSION, FRIDAY EVENING, AUGUST 13.

The General Session met at 8 P. M., ACTING PRESIDENT MCGEE in the chair.

The following action of the COUNCIL was announced by the GENERAL SECRETARY:

Special Committees were continued as follows:

1. Auditors, personnel the same as in 1897.
2. On Indexing Chemical Literature, personnel the same as in 1897.
3. On the Association Table in Biological Laboratory at Wood's Holl, to consist of: incoming and outgoing vice-presidents of Sections F and G, together with the Director of the laboratory.
4. On the Policy of the Association, to consist of: The PRESIDENT, Chairman, the PERMANENT SECRETARY, the TREASURER, together with L. O. HOWARD, T. C. MENDENHALL, W. H. BREWER and MANSFIELD MERRIMAN.
5. On Standards of Measurements, personnel the same as in 1897.
6. On Standard Colors and Standard Nomenclature of Colors, personnel the same as in 1897.
7. On the Association Library, personnel the same as in 1897.
8. On the Study of the White Race in America, personnel the same as in 1897.
9. To Coöperate with the National Educational Association, etc., personnel the same as in 1897.
10. To represent the Association, etc., personnel the same as in 1897.

PERMANENT SECRETARY PUTNAM gave a brief account of the Detroit meeting, after which resolutions of thanks were introduced by Messrs. MORSE, WITMER, FAIRCHILD, BARKER, HALL, GILL, PRESCOTT, WOODWARD and PUTNAM. These resolutions were to express the gratitude of Association to the following:

1. To the school authorities of Detroit.
2. To Principal Bliss of the Central High School.
3. To the High School students who served as ushers and to the High School Band.
4. To the Ladies of Detroit for flowers.

5. To the Ladies' Reception Committee.
6. To Mr. Joseph Berry of Grosse Pointe.
7. To the Water Commissioners of Detroit.
8. To the Park Commissioners of Detroit.
9. To the Detroit City Railway Company.
10. To the Press of Detroit.
11. To the Ladies of the Local Committee.
12. To the Local Committee and the Ladies of the Local Committee.

A reply to these resolutions was made by Mr. GEORGE WILLIAM BATES of Detroit, after which the meeting for 1897 was declared adjourned.

ASAPH HALL, JR.
General Secretary.

REPORT OF THE PERMANENT SECRETARY.

IT was considered desirable that the Association should hold its meeting for 1897 in a city easily accessible to Toronto, the place of meeting of the British Association for that year. It was therefore decided to accept the invitation received from the city of Detroit in preference to many others cordially extended by cities of the east and west.

In accordance with the custom of the Association by which a foreign guest from a national scientific association becomes a member without fee for the meeting, all the privileges of the Detroit meeting were offered to members of the British Association who might honor the American Association with their presence. Several members of the British Association and other foreign scientists accepted the invitation and became the guests of the Local Committee during the meeting.

The British Association, acting in accordance with its Constitution, invited all members of the American Association to become members of the Toronto meeting on the same footing as the old members of that Association, that is, by the payment of the annual assessment for that meeting. By special action of the General Committee, the President, Vice Presidents, Secretaries and Treasurer of the Detroit Meeting, and the past presidents of the American Association were invited to attend the Toronto meeting as Honorary Members of the British Association. A large number of members and officers of the American Association attended the Toronto meeting, where they were courteously entertained and made welcome to take part in the meeting.

The meetings of both Associations were unusually interesting and successful. At the banquet, at the close of the Toronto meeting, many expressions of international good will were uttered by leading representatives of the respective associations.

At the meeting held in Detroit in 1875 the attendance was 165 out of a total membership of 807. At this second meeting, 1897, the membership

numbered 1610 and the attendance was 292 including 21 foreign scientists, as follows: — Michigan, 52, of which 21 were from Detroit; New York, 47; Ohio, 32; Washington, D. C., 24; Massachusetts, 22; Illinois, 15; Pennsylvania, 14; Indiana, 9; Maryland, 8; Canada, 7; Iowa, 6; Missouri, 5; Connecticut, 3; New Hampshire, 3; Wisconsin, 3; Texas, 2; Kansas, 2; North Dakota, 2; South Carolina, 2; Kentucky, 2; Maine, 1; Rhode Island, 1; New Jersey, 1; Nebraska, 1; Washington, 1; Minnesota, 1; Florida, 1; Louisiana, 1; North Carolina, 1; Colorado, 1; California, 1; England, 14; Scotland, 2; Ireland, 1; France, 1; Italy, 1; Austria, 1; China, 1.

A memorial address on the scientific career of the deceased President of the Association, Professor Edward D. Cope, was read by Professor Theodore Gill. Nine Vice Presidents delivered addresses before their respective sections. Three reports of committees were presented. 241 papers were read before the sections, and 59 in joint session with affiliated societies, as follows: A, 19; B, 39; joint session A and B, 4; C, 43, and joint session with American Chemical Society, 11; D, 16; E, 15, and joint session with American Geological Society, 10; F, 18, and joint session with Association of Economic Entomologists, 25; G, 31; H, 35; I, 25, and joint session with Society for Promotion of Agricultural Science, 9.

Of the 125 members elected since the Buffalo meeting and during the Detroit meeting, 97 have perfected their membership and 9 of those elected at the Buffalo meeting and 1 elected at the Springfield meeting have also perfected their membership; 16 have paid their arrears and these have been restored to the roll; by mistake 1 was omitted from a former list but is now restored; 1 more founder of the Association has been added to the list as Honorary Life Member; making 125 names added to the roll since the Buffalo volume was published.

From the Buffalo list 20 names (including 1 founder of the Association) have been transferred to the list of deceased members; 31 members and fellows have resigned; and 266 have been omitted for arrearages; making a deduction of 317 from the list.

43 members have been transferred to the roll of fellows, and one founder of the Association has been restored to the list as life fellow.

The following is a comparative statement of the roll as printed in the Springfield and Buffalo volumes and in the present volume: —

	Springfield.	Buffalo.	Detroit.
Living patrons	2	2	2
Corresponding member	1	1	1
Members	1115	991	844
Living honorary fellows	3	3	3
Fellows	792	805	760
	1913	1802	1610
Honorary life members (founders) included in above	8	6	6

The distribution of publications since the last report is as follows: —

Memoir No. 1: sold, 3 copies.

Proceedings, Vols. 1-44: delivered to members, 219; sold, 28; exchanges, 58; duplicate copy to member, 1; duplicate copy to exchange, 1; presented, 69; = 371.

Bought, 10; received as donation, 14; = 24.

Vol. 45: delivered to members, 1353; sold, 26; exchanges, 249; duplicate to exchange, 1; presented, 5; = 1634.

Returned by member, 1; received as donation, 1; returned by exchanges, 2; = 4. Subscription has been received for 1 copy of Vol. 46.

Several important changes in the Constitution were adopted at the Detroit Meeting. The action taken by the Council at the Buffalo Meeting in relation to the publication of papers, by title only, in the volume of Proceedings, was revoked by the Council at the Detroit Meeting.

In closing this my last Report as Permanent Secretary of the Association, I am closing a most interesting chapter in my life career, — one that I am forced to bring to an end through pressure of other duties. This active service rendered to the Association for a term of twenty-five years, during which time I have attended every meeting and edited every volume of the Proceedings, has brought me into constant and close touch with the scientific men and women of the country. It has, through correspondence, afforded constant interchange of thought with scientific minds, and has been productive of much congenial companionship and many lifelong friendships. As I look back and recall the many appreciative words, courteous attentions and helpful acts of my fellow members of the Association, I feel that this period of my life will remain with me always as something more than a pleasant memory unmarred by a discord or regret.

F. W. Putnam,
Permanent Secretary.

December 31, 1897.

REPORT OF THE TREASURER.

In compliance with article 15 of the Constitution, I have the honor to submit the following report showing receipts, disbursements, and disposition of funds of the Association for the year ending June 30, 1897.

Receipts have come into the keeping of the Treasurer from five different sources, namely: first, from life membership commutations; secondly, from subscriptions to the General Fund of the Association; thirdly, from subscription of Mrs. Esther Herrman for the Association to the Pasteur Monument fund; fourthly, from current receipts transferred by the Permanent Secretary; and, fifthly, from interest on funds of the Association deposited in savings banks. The aggregate of these receipts for the year is $764.85.

Disbursements, made in accordance with the directions of the Council, amount in the aggregate to $550.00.

The details of receipts, disbursements, and disposition of funds are shown in the following itemized statement.

THE TREASURER IN ACCOUNT WITH

The American Association for the Advancement of Science.

Dr.

1897.			
June 30.	To balance from last account . . .		$5383 51
	To amount received from life membership commutation		50 00
	To amount received from subscriptions to General Fund		10 00
	To amount received from Mrs. Esther Herrman for subscription to the Pasteur Monument fund		100 00
	To amount received from F. W. Putnam, Permanent Secretary		400 00
	To amount received as interest on funds of the Association deposited in savings banks as follows:		
	From Cambridge Savings Bank, Cambridge, Mass.	$12 36	
	From Emigrant Industrial Bank, New York	46 54	
	From Institution for Savings of Merchants' Clerks, New York . . .	78 73	
	From Metropolitan Savings Bank, New York	67 22	
			204 85
	Total . . .		$6148 36

Cr.

1896.			
October 3.	By cash paid Dr. L. A. Bauer as grant in aid of magnetic survey of Maryland		$ 50 00
October 9.	By cash paid to Treasurer of Pasteur Monument fund		100 00
1897.			
June 26.	By cash paid Prof. Francis C. Phillips as grant in aid of research on properties of natural gas		50 00
June 28.	By cash paid as grant to Marine Biological Laboratory of Wood's Holl, Mass. .		100 00
June 29.	By cash paid as grant to the International Bureau of Bibliography		250 00
June 30.	By cash on deposit in banks as follows:		
	In Cambridge Savings Bank, Cambridge	$ 318 70	
	In Emigrant Industrial Savings Bank, New York	1365 07	
	In Institution for Savings of Merchants' Clerks, New York	2159 53	
	In Metropolitan Savings Bank, New York	1731 86	
	In Fifth Avenue Bank, New York . .	23 20	
			5598 36
	Total . . .		$6148 36

NEW YORK, AUGUST 5, 1897.

I have examined the foregoing account, and certify that it is correctly cast and properly vouched.

EMORY MCCLINTOCK, *Auditor*.

F. W. PUTNAM, PERMANENT SECRETARY,

Dr. THE AMERICAN ASSOCIATION FOR

For the year ending

To balance from last account		$1,178 46
Admission fees previous to Buffalo Meeting .	$ 30 00	
Assessments previous to Buffalo Meeting .	2,343 00	
		2,373 00
Admission fees for Buffalo Meeting	355 00	
Assessments " " "	2,768 00	
Associate fees " " "	90 00	
Fellowship fees	108 00	
		3,316 00
Publications sold and binding		158 13
Miscellaneous receipts		10 82
		$7,036 41

I certify that I have examined this account

CAMBRIDGE, JUNE 12, 1897.

IN ACCOUNT WITH
THE ADVANCEMENT OF SCIENCE. Cr.
December 31, 1896.

By Publication.		
On account of publishing 2500 copies Proceedings Vol. 44:		
For balance for printing	$326 18	
" extra copies addresses, etc.	87 80	
" binding	269 70	
" type writing copy, etc.	10 00	
" wrappers	11 00	
		654 68
On account of publishing 2500 copies Proceedings Vol. 45:		
For printing in part	294 41	
" wrappers	14 75	
" extras, addresses and reports	20 10	
		329 26
Back volumes bought	2 00	
Binding back volumes	75	
		2 75
By Expenses Buffalo Meeting.		
General expenses	267 56	
Printing Buffalo pamphlet	40 00	
Preparing, printing and mailing preliminary programs	58 30	
Expenses Section A	6 74	
" " C (including preliminary program)	86 50	
Expenses Section D	11 00	
" " E	6 22	
" " F	8 85	
" " G	7 50	
" " H	15 43	
" " I	5 00	
		512 60
By General and Office Expenses.		
Rent of office one year	108 00	
Printing circulars, cards, notices, etc.	86 72	
Petty office expenses and stationery	22 56	
Postage and Post Office box	331 15	
Telegraph and telephone	5 96	
Express	242 50	
		796 89
By Salaries.		
Permanent Secretary	1,250 00	
Assistant "	720 00	
Janitor	100 00	
		2,070 00
By Balance to new account		2,670 23
		$7,036 41

and that it is correctly cast and properly vouched for.

S. C. CHANDLER, *Auditor.*

INDEX.

PROCEEDINGS

OF

THE AMERICAN ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE

FORTY-SIXTH MEETING

HELD AT

DETROIT, MICH.

AUGUST, 1897.

SALEM:
PUBLISHED BY THE PERMANENT SECRETARY.
JUNE, 1898.

3 gal
174+